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THE GENERIC CONCEPT IN THE CLASSIFICATION OF THE FLOWERING PLANTS.[1]

THAT many of the most useful scientific terms defy accurate definition is a fact which can not be denied. Indeed, the recent progress of science, at least in biology, has been away from, rather than towards, any dogmatic form of statement. Many terms, which fifty years ago were smugly defined in the text-books and manuals of the time, are now, when viewed in the light of the developmental theory and from the diverse points of view of modern investigation, either well-nigh obsolete or have, through a gradual accretion of varying meanings, come to express only the vaguest generalities. This change is by no means a matter to be deplored. It is, in fact, an evidence of advancing thought. Loose and general terms are giving place to more technical and specific ones at about the same rate that the older and vaguer concepts are being supplanted by the more refined distinctions of modern science.

In some cases, however, the wide usefulness and general familiarity of terms have made them, notwithstanding some vagueness, far too valuable to discard and difficult to replace; and in these cases it is a matter of great importance that scientists should from time to time examine such terms theoretically in order that they may be applied with reasonable uniformity. An excellent example in point is the word

[1] Address of the vice-president and chairman of Section G—Botany—at the New Orleans meeting of the American Association for the Advancement of Science.

genus, so well known yet so variously used. It is in no wise my purpose in the course of these remarks to attempt to redefine the generic concept, but merely to consider the present divergence in the limitation of genera and see if there is any practical basis for a greater harmony in this matter.

Plants are classified according to their degrees of likeness or dissimilarity to each other. In likeness we see evidence that certain plants are related, and by related we mean that they are sprung from a comparatively recent common ancestry. Likeness, it is true, no matter how close it may be, is not an absolute proof of near relationship; but, since in most cases it is impossible to trace conclusively the ancestry of existing species of plants, their degree of similarity furnishes almost the only evidence available in regard to their relationship. I need scarcely say that by likeness is here meant not mere habital resemblance, but the sum of all the morphological, anatomical and physiological similarities between the plants compared. Bearing these matters in mind, we may roughly describe a genus (when pluritypic) as a group of species which from likeness appear to be more nearly related to each other than they are to other species. But so varying are the degrees of similarity and so diverse is human judgment regarding them, that such a definition offers only an exceedingly vague basis for a uniform classification.

Indeed, the practise of different botanists in the interpretation of genera has been so multifarious that many persons have become sceptical as to the real existence of such groups in nature and are accordingly inclined to treat the whole subject of generic classification as a mere matter of utility, a sort of division of the plant world into sections of convenient size by confessedly artificial lines analogous to the parallels of latitude and meridians of longitude. But much may be said for the objective reality of genera, at least in certain families. Who will wish to contend, for instance, that such tolerably uniform groups as *Lupinus, Aquilegia, Delphinium* or *Carex* are not far more distinct natural categories than are many of the relatively vague and ill-defined species of which they are composed. There can be no doubt whatever that in many families clearly definable genera exist and have been duly recognized. In other families or parts of families, however, the living species do not fall into sharply limitable groups, but are by character either somewhat isolated or more often exhibit highly complex cross affinities rendering any simple or convincing classification into genera impossible. It is in such groups that uniformity of classification is most difficult to obtain, for it is in them that individual judgment has the widest play.

There are two very different methods of treating genera. One is to lay much stress on the idea of a generic type, that is to say, a species of the genus in question, which is supposed to fix the generic character and pass as a sort of sample or gauge, by comparison with which other nearly related species are to be judged. If they appear to agree with this type-species in the essential characters, namely in those points which in the particular family concerned have come through accumulated experience to be considered of generic value, the species are considered congeneric with the type and receive the same generic name. The other mode of treating a genus is to endeavor not so much to group the species about some historic type as to indicate the precise circumscription of the genus by pointing out as clearly as possible just how its species as a whole differ from those of related genera. A species has sometimes been not unaptly referred to as an island in a sea of death. Carrying out this simile

as I believe Mr. Cook has done, we may for purposes of illustration consider a genus as a sort of archipelago of such islands, the members of the group being separated by rather shallow channels, while the archipelago is itself separated from other similar groups of islands by deeper channels. It has been urged that a type-species, when accurately defined, furnishes a sort of latitude and longitude of one island in the archipelago and thus gives one of the best means of locating the group as a whole. To a certain point this is undoubtedly true, yet the information, however important, is by no means sufficient for purposes of classification, for it is obvious that to know the precise position of an island in an archipelago gives no clue to the shape and extent of the group. To acquire this latter knowledge we must measure and sound the intervening channels. Let us carry out our figure a little further and agree that the breadth of a channel indicates the extent of actual difference between the plants in question, while the depth of the channel indicates the fixity of this difference. It is obvious that islands may be far apart and yet be divided only by very shallow water, the space between them being from its inconsiderable depth very likely to be more or less filled with spits, shoals or sand bars. Similarly, as we all know, two species may in their typical forms be very unlike, yet exhibit such affinity that they are more or less connected by frequent intermediates, hybrids or atavistic forms. On the other hand, two islands may be very near together yet separated by an exceedingly deep channel, in which any intermediate bars, shoals or islets are quite impossible; and in like manner two species may exhibit close habital similarity and yet maintain their technical distinctions with perfect fidelity.

It is, of course, these deeper channels, these natural intervals, no matter how narrow and difficult to find, that should be diligently sought as yielding the most satisfactory limits of a genus. It is by the relative or complete absence of intergradation that we must recognize differences of high antiquity and profound classificatory significance. It is not by the visible extent of the differences, but by their constancy that their importance is to be measured. This is, of course, no new thesis. It has been reiterated, time and again, in one form or another, by many distinguished biologists. Yet it is a principle habitually disregarded by many systematists, and to its neglect is due a large part of the annoying diversity in current classification. Large genera are daily being divided and new ones artificially created on the basis of differences which are regarded as important solely from their magnitude or conspicuous character and with scarcely a thought as to their constancy. But it is easy to see that botanists, who confound differences of degree with those of kind, or lay great stress on the wide divergence of certain type species and fail to take into account the species which are intermediate, are like reckless mariners who estimate the depth of a channel solely by its breadth.

The great majority of new generic propositions rest upon the examination of a comparatively small number of species; indeed, a considerable part of them are brought out in works relating to some geographically restricted flora. In such cases, it is a practise all too common to treat the newly proposed generic segregates as though they were made up solely of their representatives which chance to inhabit the particular region studied. In many instances this enables the writer to define his genera with a specious definiteness, which may appear very convincing to those whose botanical activities are restricted to the same local flora, but which, when viewed in the light of a broader knowledge of re-

lated extra-limital species, is quickly seen to be the merest artificiality.

This introduces a subject which I would earnestly emphasize, namely, that those who attempt to alter generic lines should always take a broad, a cosmopolitan, view of the group concerned. To do so, may, it is true, involve great difficulty. It is a relatively easy matter to divide into groups two or three dozen species of any one of the great genera like *Euphorbia, Solanum, Cyperus, Eupatorium, Polygonum* or *Astragalus.* It is a very different task to examine all known species of any of these huge genera and show that they may be divided into definable and mutually exclusive groups; but it should be perfectly evident that nothing short of a definite disposition of all the component species of a genus can be construed as a satisfactory and scholarly generic segregation. It would probably be within the bounds of truth to say that wide-reaching divisions of large genera have within the last decade been frequently undertaken without the examination of more than a quarter or in some instances a tenth part of the species involved. Such attempts may be compared if we return a moment to our marine simile, to the guesswork of a negligent explorer, who, finding an archipelago entered by a deep inlet or bay, should, after sailing a short distance into it, conclude that the channel continued at the same depth and in the same direction through the rest of the group, and who should, therefore, record such a clear channel on his chart, leaving to future mariners who attempt to sail by his map the unenviable task of discovering by sad experience the real course and depth of the channel. Neither such a chartographer nor such a botanist is likely to enjoy long a reputation for accuracy or scholarship.

On the other hand, it may be urged, with some plausibility, that many large and traditional genera contain very unlike elements and that those writers who clearly perceive this should not be obliged to maintain the heterogeneous aggregate, even though they may not be in a position to go into the great task of examining all its foreign representatives and deciding in which component group each should be placed. It may be argued, further, that to impose such a burden of work as a prerequisite to every more sweeping generic change would be to retard very greatly the progress of classification. These, however, are mere excuses for hasty and superficial work. In fact, just such expressions as 'composed of very unlike elements' are exceedingly apt to be based on differences of magnitude rather than those of constancy. Persons examining our indigenous species of *Polygonum* might very naturally suppose the sections Persicaria and Avicularia distinct genera. It requires a knowledge of the Old World species to perceive how untrustworthy are the distinctions by which these subordinate groups are separated. The caryophyllaceous groups *Alsine* of Wahlenberg and *Melandrium* of Röhling are commonly maintained as genera by writers of central Europe and if only European species are considered these might seem fully worthy of generic rank. It is after a study of Asiatic and American species that the alleged generic distinctions are seen to be weak and inconsistent. It is an easy matter to separate sharply our few species of *Oxalis* into groups on the basis of homogony and heterogony, on the color of the petals, and on the nature of the rootstock. It is a very different task to arrange clearly in these proposed genera the species of Africa, in which yellow petals are sometimes associated with a bulbose base and purple petals with elongated leafy stems, in which heterogony bears no definite relation to color of petals, and in which

scaly rootstocks and bulbose base are actually found in the same individual.

The genus *Hexastylis* of Rafinesque has been recently restored and separated from *Asarum* on the ground that the former has a superior ovary, distinct styles, and filaments shorter than the anthers, while in the latter it is said that the ovary is inferior, the styles united, and the filaments are longer than the anthers. This might seem to be an excellent generic division were it not for Asiatic species of *Asarum* in which an inferior ovary is combined with distinct styles and nearly sessile anthers. Examples of this sort might be almost indefinitely multiplied.

In all these cases the generic segregation is weak, ineffective and unscholarly because it rests on incomplete observation. It is, moreover, distressing to see what a strong prejudice there is on the part of many investigators against taking a broader and more cosmopolitan view of plant classification. If genera appear to be good in a particular region, that is regarded as quite justifying their maintenance in floras treating of that part of the world. This tendency toward insularity is to some extent perceptible in the work of different European nations. It is far more noticeable in the growing separation of the taxonomic work of the eastern and western continents. It is obviously the outcome of the narrowing influence of specialization and should be vigorously combated, for it is seriously threatening not only the uniformity and harmony, but the normal progress and dignity, of our subject as a whole. For it is quite evident that were it to be once admitted that generic limits rest not upon species of the world but are to be fixed in each flora merely according to their chance representatives in that flora, it would introduce a chaos into classification as ridiculous as it would be unnatural. Two genera might be justifiable in the limited flora of Connecticut, which would completely merge were the other New England species considered; or two genera might present marked differences in New England, which would intergrade in the middle or southern states. It is obvious that if we are to obtain any measure of uniformity or stability, genera must be founded not on local, but on a cosmopolitan basis.

It has often been suggested that excessive splitting of genera has been due to personal vanity of authors who enjoy creating the new binomials involved. However this may be, I much doubt whether any such unworthy motive has played great part in the matter, but infer that many authors share the view of Rafinesque that large genera are unwieldy things; that it is a very difficult matter to prepare a good key for such large numbers of species and that classification is rendered simpler and clearer if the genus can be broken up into fragments of convenient size. In regard to this it may be said that the subdivision of the genus into subgenera or sections accomplishes precisely the same end with no confusion of generic nomenclature. Different minds may work in unlike manner when confronted by the difficulties of identifying plants. Personally, I should very much prefer to have the difficulty at one point rather than at two; that is to say, I should rather have generic lines drawn so widely that genera would be pretty definite and readily recognized, in the manner, let us say, of *Cyperus, Astragalus* or *Euphorbia* in the broader and long traditional sense. The recognition of such genera requires little or no mental effort on the part of a botanist of any training. The attention is left free for the specific identification and this may be undertaken with a happy confidence that all the species likely to come into question will be found in the same group and under the same generic name. These species may be inconvenient-

ly numerous, but at least one is not disturbed by any lurking doubt whether, after all, he has got the right genus. I can see no real simplification in having *Mariscus* and sundry other vaguely marked sections of *Cyperus* treated as separate genera, so that in identification one must first struggle with the generic key and then, haunted by some misgivings as to his success in this matter, proceed to the specific key. As I have said, minds may act quite differently in this regard, but clarity of classification is after all a very artificial consideration and it is dearly bought if secured at the expense of misleading statements. Therefore, I can not believe that a writer when dealing with a limited flora is justified in stating that two genera are distinct if he is wilfully or carelessly neglecting their more or less complete intergradation in some other flora.

It may be thought that the sort of world-wide perspective which is here advocated is a thing well nigh impossible; that a writer who attempts a flora, for instance, of the United States will have his hands quite full with the difficulties of his already herculean task and that his progress would be slow indeed were he forced to estimate each generic difference by the large standards of the world's flora. Here I must use a much-dreaded word, quite a bug-bear of many a self-reliant scientist, namely, *authority*. It is indeed quite impossible for any one of us to repeat the accumulated work of many decades which has led to the recognition of hundreds and thousands of genera. We must accept at least a large part of them on authority. The absolute necessity of this is so self-evident that it needs no justification. I am not advocating any slavish submission to the opinion of others, nor minimizing the importance of verifying and in every possible way correcting previous work, but merely urging a wholesome respect for the opinion of those whose intensive monographic work or broad cosmopolitan outlook has given them an exceptional opportunity to see just where generic limits may be most naturally and profitably drawn.

Fortunately, the genera of flowering plants have been comprehensively treated in two or three works of high excellence. These sources of information are sufficiently complete and recent to be of the greatest service to writers on restricted floras. In addition to such general guides there exist for many families very detailed monographs embodying the expert opinions which are the result of long and critical study of all members of the particular affinity concerned. It is not to be denied that such monumental works exhibit a considerable measure of diversity in the interpretation of genera, but this is not wholly a misfortune since it permits a certain elasticity in classification and enables a certain selection according to varying judgments. Furthermore, however great the divergence may be in these large monographic and cosmopolitan works, it is relative uniformity compared with the state which would soon obtain were genera to be defined in each flora solely according to its local representatives.

One of the most unhappy tendencies observable in modern classification is a gradual letting down of standards, a feeling that if a few ill-defined genera are to be found in a particular family, the others should in the interests of a sort of specious symmetry be cut up until all are about of the same degree of vagueness and uncertainty. When an author who tends to excess in dividing genera feels called upon to assign a ground for his action, it is in nearly all cases that the segregates he is making are quite as good genera as many which already exist.

This process of taking the poorest existing work of others for a guide or as a

sample of what is permissible might obviously be carried on forever. In the great majority of cases this type of classification is the work of authors who have quite lost sight of the fact that it is not the magnitude but the constancy of the differences which is of real significance in classification. There is, for instance, no more dangerous theory than that because a particular trait, let us say the presence of a corona, forms an excellent generic distinction in one family it must necessarily be of generic significance in another.

The frequent occurrence in botanical literature of such expressions as 'a generic difference,' 'distinctions of generic rank,' etc., seems to indicate a more or less widespread feeling that differences of a particular kind or magnitude, or relating to special plant-structures, are in some way recognizable as diagnostic characteristics of generic value as opposed to those which can be used merely for the separation of species or varieties. There is a common idea, for instance, that a difference of floral structure, or especially one of fruit or seed, is almost infallibly trustworthy in the separation of genera. This belief has certainly the justification that distinctions in the essential parts of the flower or in the fruit are much more apt to be fixed, are at least far more constant, than those of habit, foliage or pubescence, since the latter appear much more subject to modification with varying environment. Nevertheless, this theory, as a rule of classification, may easily be carried too far. When carefully examined, very few differences in the structure of flower or fruit will be found to be invariably of generic value. Nearly all appear at some point in the plant system to be in a fluctuating state. For example, the position of the cotyledons, although possessing a high value in separating many cruciferous genera, completely breaks down in the genus *Lepidium*, where in plants of the closest habital similarity accumbent and incumbent cotyledons are found. In the Rosaceæ the unlike adnation and connation of the floral parts furnish in some portions of the great family excellent generic and even tribal distinctions. In other rosaceous genera, however, the same diversity of adnation and connation occurs in species which are obviously of such close relationship and so connected by intermediates that their generic separation is by no means convincing. Many other instances of this kind might be cited to show that a difference having great classificatory significance in one place may be almost valueless in another.

It is fair to inquire how we know this. If, for example, genera of the Cruciferæ can be readily separated by the position of the cotyledons, why is it not after all the most logical course to treat *Lepidium virginicum*, with its accumbent cotyledons, as constituting a genus distinct from the other species of *Lepidium* in which the cotyledons are incumbent? There are two pretty obvious reasons against this. One is that in *Lepidium* there are other species which have transitional cotyledons, exhibiting various degrees of obliquity. In the second place, the accumbent cotyledons of *Lepidium virginicum*, although a striking character, are unaccompanied by any other difference of moment. From this fact we may reasonably infer that in this particular group the difference in the position of the cotyledons is of relatively recent origin, for it has not had time to become correlated with any other trait of significance or constancy. This brings us to a matter of great practical as well as theoretical importance in classification, namely, that few, if any, genera carry conviction as natural groups, or, to be more precise, naturally delimitable groups, unless they can be separated by more than one feature. The ideal genus is certainly one in which several distin-

guishing traits are constantly associated. When limits are properly drawn it is certainly true that a very large number of such ideal genera exist. Unfortunately for the peace of mind of the systematist, however, there are considerable series of species in certain families, which quite defy classification into genera of this sort. They are groups in which we are forced into accepting a far less satisfactory type of generic division, and in some cases it is necessary to make the most of a single character. Of this fact, the genera *Arenaria* and *Stellaria* furnish an excellent example. So far as I am aware there is no technical distinction between these genera except in the petals, which are entire in *Arenaria* and bifid in *Stellaria*, and this difference becomes very weak in certain species in which the petals are merely emarginate, yet I doubt if any systematist at present wishes to unite these two large and traditionally maintained genera.

It is thus clear that by common consent we are willing to preserve for the sake of practical convenience certain familiar genera, even though precise technical grounds are lacking and a few intermediates occur. In other words, there is an historic element which will obtrude itself into our classification. Its influence is obviously opposed to a truly natural and symmetrical system; nevertheless, much as it may be deplored on theoretical grounds, it is an undeniable fact, and as such demands careful scrutiny and consideration.

Perhaps no large family shows the importance of the historic element in generic classification better than the Compositæ. The lack of sharp boundaries between such genera as *Aster*, *Erigeron* and *Conyza*, *Solidago*, *Aplopappus* and *Bigelovia*, is patent to all whose botanical experience extends beyond the limits of some local flora, yet so far as I am aware no botanist has been willing to accept a recent proposition to merge several of these genera into one. The genera have through long recognition become a practical aid in classification. They are groups into which at least a very high percentage of the component species may be naturally associated, those of intermediate character being, notwithstanding their undeniable existence, relatively little in evidence.

But it may be asked whether the recognition of such somewhat ill-defined genera is not quite at variance with our fine theories that genera should be groups capable of clear and mutually exclusive definition, archipelagoes separated by deep and safely navigable channels. Certainly this is quite true, but I must beg you to notice that I am in no wise urging any one to recognize weak or poorly limited genera; I am merely stating the undeniable fact that certain groups of this sort are now so widely recognized as genera that we have no choice in the matter. *Aster*, *Erigeron*, *Conyza* and the like have become established facts in classification. They fail to reach our ideals, but this does not of necessity mean that we must let down our standards elsewhere. Attention may be called to the fact that when they were first proposed these genera were, so far as they were then known, quite as distinct as could be desired; their present vague and merging state being due chiefly to intermediates discovered long after the founding of the genera. With the increase of knowledge these genera which once seemed distinct have grown together, the process has been a gradual one and at no time has it seemed desirable to abandon the idea of these genera, although it has become increasingly evident that it is to a great extent artificial. This is one way in which the historic element has entered into our classification. We may readily admit that the authors establishing these old and now merging genera were in most instances justified in doing so, because

according to the knowledge of their time the groups seemed pretty sharply separable, yet that does not mean that we should take those merging genera as a model for our own work. It is, for instance, a very different matter at this time to pronounce the great and long united genus *Astragalus* a complex of distinct genera, for with our present knowledge of the gradual transitions between these alleged segregates of *Astragalus,* their treatment as separate genera involves a sort of suppression of facts.

It is for this reason that writers who divide large genera on the ground that the component parts are just as good genera as many which are traditionally maintained are doomed to the disappointment of seeing their propositions neglected. We may readily pardon the older writer who in his limited knowledge of species made some incorrect inferences as to generic lines, who pointed out the probable channels about his archipelago on the basis of a conscientious but necessarily restricted exploration, made, let us say, with a still ineffective sounding apparatus. It may even, for purposes of geographic orientation, be worth while to let his hypothetical channels remain on our charts, their shallowness and danger being duly indicated. It is quite another matter when we are directed by modern writers to record such channels on the chart at places where we know that they do not in reality exist. But it may be asked why, if it is sometimes desirable to leave the record of an old error, may not the very similar new proposals be equally useful for purposes of geographic orientation. If it is worth while to let old and now merging genera stand, why not make new ones of the same kind? But this end can be accomplished equally well by the use of subgenera or sections, and that too without our seeming to indicate that differences are more constant than they really are, without our being obliged to record a navigable channel where none exists.

The division of large genera into several smaller ones is commonly accompanied by a certain loss about which little has been said, the obscuring, namely, of the existence of the larger affinity which had been indicated by the old and more comprehensive genus. However diverse the elements of *Œnothera* may be, it can not be doubted that they form a recognizable whole, of which the constituent species are more nearly related to one another than they are to *Epilobiums, Gauras* or to species of other onagracious genera. In other words, *Œnothera* in its comprehensive sense is a natural, although perhaps rather loose group. If we show the narrower affinities, by the use of subgenera, we still have the word *Œnothera* left to cover the larger relationship. This is a matter of importance, for it gives the student at once the information that the affinities of the parts of the genus are closer than those of the genus to other genera. If we give up the old genus *Œnothera* and substitute a group of small component genera, we lose sight of the larger affinity and our classification is accordingly less clear and rich in its statement. It has suffered a distinct loss in the abandonment of the genus *Œnothera* in the larger sense. An added disadvantage is that we are making the constituent groups, although they are of a nearer affinity and clearly belong to a subordinate rank in classification, appear as if they were coordinate with other genera which are still treated in the larger way. It is clear, therefore, that the division of a loose but more or less natural genus is attended by some disadvantages quite unconnected with any considerations of sentiment or temporary inconvenience.

We have seen that the difficulties of classifying plants in a really natural and logical way are somewhat increased by the

involuntary and well nigh necessary admission of a certain historic element into our systems. There is another source of this artificiality, besides the temptation to allow poor genera to stand, on the ground of long usage. The relation of a genus to its name is a matter which exerts no small influence in this regard. The attempt to determine which of several names is to be retained for a given genus constantly forces us to consider the historic basis on which the genus rests and to attach its name to some species or group of species to which it was first applied, to determine, in other words, what was the type of the genus, and to maintain the genus in such a way that it may always be true to its type. While sympathizing to a considerable extent with those botanists who desire to place our nomenclature upon a more secure basis by attaching the names to recognized types, I feel that the methods employed will have to be very cautiously applied or they will tend greatly to increase the artificial element in our system. The historic type is not a natural thing; it is merely that particular form of plant life which was, often quite by accident, first discovered and, therefore, first received the name which it bears. Later discoveries often show that this first species of a genus is by no means of a typical, or, as one may say, central character. It is often quite peripheral, perhaps even an aberrant or outlying member of the group to which it belongs. However important the historic type may be in nomenclature, it is obvious that it is of no particular significance in classification, and any employment of the type method in the determination of proper names must not on any account be permitted to exercise any influence in classification. The word type itself is decidedly unfortunate as thus applied to what is often very far from being typical. In this as in some other phases of taxonomy it is of the greatest importance to keep it clearly in mind that nomenclature, although very necessary to classification, is a thing wholly apart from the classification itself. It is, furthermore, quite evident that nomenclature should be subservient to classification and that the clearness and accuracy of classification should never be sacrificed in order to give beauty or symmetry to any system of nomenclature.

I have now stated my premises and perhaps you are looking for some conclusions, or possibly some practical suggestions as to the best way of obtaining a greater harmony in the matter of generic classification. The difficulties of the problem are quite apparent. The limitation of genera has always in the past rested on individual judgment and it must continue to do so in the future. There is no way of making all people think alike on a subject so intricate and I am by no means certain that complete unity is really needful or desirable. The fact remains, however, that, although the genera of the flowering plants have now been scientifically studied for about two centuries, there is at present in America, at least, a degree of diversity in their interpretation which is rather discouraging. It is disheartening because it is impossible to see in it any real progress toward a well-rounded and satisfying system, which will win the confidence of the professional botanist, give uniform training to the student and command the respect of our colleagues in other branches of science. From this, I think that it is perfectly clear that botanical systematists have certain imperative duties in regard to this subject. These duties are, in the first place, great caution in making changes, and in the second place, a feeling of obligation, when these changes seem necessary, to state the reasons for them so clearly and forcibly that they will appeal to all thoughtful and discriminating workers in the same field. The burden of

proof should always rest upon the writer suggesting the change. It is rather surprising to notice how lightly this matter is taken by some, who attempt sweeping changes. It is by no means rare to see a few habitally similar species of a large genus split off and set up as a new genus with scarcely any attempt to give accurate definition to the new group or tell just what traits are of diagnostic value in separating it. The authors of such work indolently and carelessly shift the burden of proof upon others. Their statements regarding genera are scarcely more than hypotheses, but unhappily they are expressed not as theories or mere conjectures, but as facts. This, however, is not the worst type of publication on this subject, for it is even more misleading to assign generic characters as in the case of the segregates of *Oxalis*, for instance, which do not hold good. Do not understand me to say that authors have been intentionally misleading, for I do not believe that to be the case. I merely mean to say that some writers who have made rather free changes in generic classification have taken so lightly their responsibilities in doing so and have felt so little the obligation to present any complete or scholarly proof of their dogmatically stated conclusions, that they have been tempted into rash and hurried assertions, which are in many cases decidedly misleading.

It may perhaps be thought by some that this is unduly hard on writers who have intentionally adopted a lower grade of generic classification than the somewhat ideal one described a few moments ago, who believe in the practicality of treating as genera minor groups of allied species, although it is by no means maintained that these are sharply defined or non-intergrading. To this it may be said that these minor groups, being of a different classificatory rank from the larger long-established genera, such as *Cyperus, Œnothera, Astragalus, Ranunculus* and the like, should not in any natural or well-devised system be treated as coordinate with them and bear the name genus, this name having been applied to groups of a superior rank. This seems to me a very important matter. For if it were considered proper to apply the term genus to smaller and smaller groups with more and more vague definition, there is surely no end in sight and no real progress toward a definite and reliable system. To draw an illustration from another science, it may be noted that each generation sees an added perfection and accuracy in apparatus for physical measurements of space, time and gravity, but the physicist does not feel it necessary on this account to shorten the meter scale, make his clock run faster, or file down the weights of his balance. His old constants remain as treasured acquisitions and it is by careful reference to them that his progress is made.

What we need in botanical classification is a series of such constants, a number of graded categories which can be generally endorsed and properly respected. Standards as definite as those of the physicist are, of course, quite unattainable in dealing with the variable and often intergrading groups of organic creation. But where absolute accuracy and uniformity are impossible, we should the more diligently seek to preserve such standards as exist. As has been pointed out there are but few families of flowering plants which have not been comprehensively treated by monographers who so far as their particular group was concerned have been in a position to see pretty clearly where it was best to draw generic lines. While it must be admitted that there are many minor differences in the generic concepts exhibited in the scholarly and monumental works to which I here refer, yet they establish a good usage, which on the whole has a consider-

able measure of uniformity and which goes far to establish the rank of such categories as genus, species and variety. This fact is clearly shown by the contrasting work of those free-lances who, armed with the less effective weapons of a more restricted knowledge, have in doing independent battle upon the difficulties of generic classification followed other tactics and set up new standards. I doubt if they have realized how quickly and fully the personal equation is recognized in regard to their work, or how generally even the amateur and layman grasp the fact that their generic and specific propositions are not up to the standard. No one can change the temperature by making the degrees of his thermometer smaller. Least of all is it possible to make people believe that the shortened degree is as important as the longer one. Time spent in this mere letting down of standards and shifting of ranks is worse than wasted. The process is annoying and confusing, for to the natural difficulties of generic classification plus a certain inevitable historic element of artificiality, it superimposes the most awkward and irritating difficulty of all, namely, the personal equation.

Let us get something done and not spend our time in endless and profitless strife about first principles, thereby bringing confusion into what may be regarded as fairly well established already. There are limitless fields for further profitable work in the finer classification of the flowering plants without perpetual tampering with the boundaries of important and long-studied genera—a type of activity very prone to sink to the level of a mere juggling with names. Having said so much against generic changes of a superficial character, I fear some of my hearers may get the impression that I am opposed to generic changes in general and perhaps even to the further investigation of generic limits; but this is in no wise the case. There is certainly great opportunity for further and very profitable study of generic classification. The genera of several families, as for instance the Cruciferæ, are in many cases pretty artificial groups. We need much further knowledge of the relationships of the species concerned. Let those who wish to be of real service in this matter give us what we so much desire, namely, additional light upon the ontogeny, embryology or finer anatomy of these species, sources of information sure to yield data of high classificatory importance.

In closing let me urge that, while we remit no effort to secure further light on this subject, there should be a general agreement to treat the accepted and traditional interpretation of large and important genera as sacred and binding until we can furnish definite and convincing evidence that change is needful, and that for the welfare and dignity of our science, all should unite in opposing changes of the artificial sort, which consist merely in the shifting of ranks and modification of standards.

B. L. Robinson.

Harvard University.

Investigations and Commercial Tests in Connection with the Work of an Engineering College.[1]

In any school it is necessary, in securing the best efficiency in instruction, that the professors shall be able to speak with authority on the subjects which they teach. In technical schools those who teach the practical engineering subjects can not speak with authority unless they have had practical experience. Investigations and commercial tests may serve to give them this practical experience, and the question naturally arises—is it a good policy for pro-

[1] Address of the vice-president and chairman of Section D—Mechanical Science and Engineering, at New Orleans, December 29, 1905.

fessors to conduct such work in connection with their regular college duties?

Let us consider the various ways in which a professor in an engineering school may acquire the practical experience which is necessary in his work.

First, he may be called to a professorship from the practical field.

Second, after teaching for a time and finding how necessary a practical experience is in his work, he may turn to the practical field, and then return to teaching.

Third, he may undertake practical work in connection with his college duties, and gain his experience in this way.

Each method possesses its own advantages and disadvantages. Starting with the first, it must be admitted that many of our best instructors have entered the teaching line after they have had experience in the practical field. Such a man has an advantage in being able to make use of this experience immediately, when he starts in at his teaching work. There is a disadvantage, however, in the fact that should he have secured a mature experience in the practical field, he will necessarily be no longer a young man, and it may be hard for him to teach and to properly adapt himself to the theoretical part of his course. Again, if he has made a marked success in the practical field his financial reward may be so great that he would have to make a considerable sacrifice in this respect should he turn to teaching. There are some men who, from their love of teaching or through the influence of high ideals, have been willing to do this, but these are few, and the college obtaining such a man is, indeed, fortunate. This side of the problem is a serious one from the standpoint of the college, because the man it would like to get may be beyond its reach, and those available may have made only a partial success in the practical field.

If a young man with a limited practical experience becomes a teacher, this will be of assistance to him, but not as much as the more mature experience of an older man. In either case, a professor should not assume that, having had a practical experience, this is all-sufficient, and that it simply rests with him to base his teaching on the results of this experience. To keep in touch with what is going on it will be necessary for him to spend much of his time in studying what is being done in the outside field, or to resort to the third method, and do practical work in connection with his regular college duties. If he does not do this he will soon fall behind-hand, and the efficiency of his instruction will be decreased.

The advantages of the second system of securing a practical experience, where the professor leaves the teaching field, takes up outside work, and then returns to teaching, are that during his practical career he will be very much alive to the points he should look into, and, furthermore, if he returns to teaching he will possess the advantage of having experience both as a teacher and as a practical engineer. We all know that there is much more to the right sort of teaching than a thorough knowledge of the subject to be taught, and that the old saying 'a man must be born a teacher' contains much truth. It is, indeed, just as necessary that a successful teacher shall have the right qualities as an instructor, as that he shall possess the necessary knowledge. The main disadvantage of the second method is that if a professor makes a success in the practical field his financial reward may, as already stated, be so great that it will be hard to tempt him back to teaching.

We will now take up the third method, where a professor obtains his practical experience by conducting outside work in connection with his college duties. In the first place, let us consider the subject from

the standpoint of the man himself. If he is an enthusiast, this method of obtaining an experience will lead to harder work on his part than either of the others. This assumes that his college work is not neglected, and that by doing the outside work he is making his course at all times better and more useful to the students. In this connection something may be said about the methods of teaching in engineering schools. I believe that the one proper way to teach an engineering subject is for the professor at the head of a department to get down to hard work with his students, and to know each of them so well that he is thoroughly acquainted with their personal characteristics. Certain parts of his course can be turned over to assistants, but the moment he avails himself of the assistance of others to such an extent that he is no longer in close relationship with the students, his efficiency in instruction will be decreased. In other words, the professor must exert himself to the utmost to secure the best results with his students, and when this is done, there is no harder way to gain a practical experience than by the undertaking of outside work, and if the professor is an enthusiast there is often much danger of his breaking down under the strain. On the other hand, if he manages to secure an experience which will give him the reputation of being an authority in his field, he usually will make the best sort of a teacher, and be a credit to the college with which he is connected. All this assumes that the classes are not too large, and I am a thorough believer in comparatively small classes, and also that his teaching roster is so arranged that he will have time available for the outside work. It would certainly be unfair to expect a professor to gain a reputation in either research or commercial work if his time is so fully occupied with class work that, with the exception of his summer vacation, there would be little time except the evenings in which to do such work. One must not be misled, however, in thinking that if some definite time, say a certain number of days each week, is set aside for the purpose that this will make it an easy matter for a professor to undertake practical work. If he is an enthusiast he will find that he must often stand up to his task night after night, and unless he is willing to do this it is folly for him to undertake to gain an experience in the way indicated.

From the standpoint of the college, allowing the professors to undertake any and every sort of work to gain a practical experience is a most dangerous one. There is a tendency in many cases to do work for which the professor has not the proper capacity, and in this way he may throw discredit on his college. There is nothing that will so greatly damage the reputation of a professor, and in turn of the college with which he is connected, as the issuance of a report which shows him to be incapable of properly coping with the subject, or which is in the nature of an advertisement for this or that machine or commodity. The outside work undertaken by a professor should be that of a scientific or strictly engineering type. In much of the work his reports simply recite facts deduced from various test data, whereas in others it is necessary for him to render an opinion. There is danger in arriving at false deductions as well as in advancing false opinions, and unless the professor can be thoroughly trusted, it would be better to cut out all such work. There is also a great danger of a professor bringing disrepute to his college if he is careless in his testimony as an expert. If his testimony in this line is wilfully misleading, or if he depart a hairsbreadth from truth, it were well that he had never been given the chance of lowering the dignity of his profession. On the other hand, a strictly

honest and able man may bring credit to his college by his success and acknowledged fairness in this same line of work.

The advantage to a college in having its professors do research and outside work is that what reflects to the credit of the professor will reflect to the credit of the college. Furthermore, the college will be looked to as a source from which an unbiased opinion can be obtained, and in maintaining this standard it will be fulfilling a high and useful mission. The results of the investigations may be made the subjects of scientific papers to be read before the various societies, and any reputation that a professor gains in this way will benefit his college. Furthermore, the college gains through the acquisition of much of the apparatus used in the investigations. The college will also be the gainer financially, because if a professor is allowed to do professional work what he receives from this source really pays a part of his salary. Aside from a financial gain in this way, however, it is a question whether a college should endeavor to secure any great financial return from such work. There should be enough earned directly by the college to pay for the use of the apparatus, the wear and tear, etc., and for providing a fund for the purchase of new apparatus for the work; but aside from this, it is my opinion that the college need not be the financial gainer. I say this because if too much stress is laid on the money-getting side of the problem, it will not be possible to do the right sort of work, and to do it in a proper way. A party may be willing to pay a large sum for an investigation of a project where it is evident that no matter what results are obtained they will not add to the scientific knowledge of those undertaking the work, and where it is also evident that all that is wanted is the name of the professor, and that of the college with which he is connected, for promoting the project. Such work should never be undertaken, no matter what the commercial returns may be. On the other hand, a poor inventor may desire to have a test made on some machine which he has constructed, and in which there appears to be a possibility of development, and such a test might well be undertaken although it may give no financial return. The proper sort of commercial work to do in connection with a college is that of a research character, or where scientific results are obtained. It will be found that when the work is limited to this sort that it often cuts off much that is of a less desirable nature, but in which the financial returns would be greater.

In all that has been said so far it is assumed that it is necessary for certain professors to have an experience in the outside field. I do not think any one will question this. One is apt to get into a rut in teaching and to have his mental horizon narrowed so that he can not perceive his own faults, and there is no better way of expanding this horizon than to be a coworker with practical men. The main advantage that a practical teacher has over one who is purely theoretical is that he can make his course interesting by the introduction of practical examples, and in this way incite the students to study intelligently, which is the real measure of his success. He may, however, make the mistake of introducing too many practical details into the course. It is essential above all that the fundamentals of the subject shall be mastered, and the true use of practical problems, aside from securing the interest of the class, is to show that their solution is based on a few broad underlying principles. A practical man may also make a mistake in thinking that his way of looking at a problem is so simple and straightforward that it will surely be grasped by the students. In teaching, however, he will find that after

presenting a subject in what he considers a way that all must understand, there will be many, possibly the majority of the class, that have failed to grasp his reasoning. Men's minds work differently, and the path taken to arrive at the understanding of a problem will vary, so that unless a subject is presented in several ways the explanation given by a professor may fail to fall into the line of thought of many of his students, and he will be disappointed in the results obtained.

It is a fact, however, that there are many professors who have had little or no outside experience. This often occurs where a young man enters the profession of teaching directly after his graduation, and is placed in such a position that it is impossible for him to undertake any practical work. Such a professor may make the best sort of a tutor, and may be most successful in imparting the fundamentals of a subject, but when it comes to being put in charge of a practical engineering department, it is here that his lack of experience will be very much felt. In a certain sense it is unfair for a head professor to secure the services of a recent graduate and keep him continually at teaching so as not to allow him to gain outside experience. Much thought has been put on this phase of the problem. Mr. Walter C. Kerr, in connection with his work as a trustee of Cornell University, has been in favor of the plan that a professor be thrown upon his own resources and be compelled to work in the practical field one year out of seven. This might appear all right from a business standpoint of the college involved, but how about older professors who have worked long in the teaching line, and have not had the necessary outside experience to qualify them for taking a position in the practical field? It might be very inconvenient for such a professor to have to accept a nominal salary in order to gain experience, but this certainly would follow if he had been so loaded down with teaching that he was given no opportunity to work at the practical side of his profession. On the other hand, if a professor through hard work and diligence has obtained experience in the outside field, while he is a teacher, and is forced, at the end of seven years, to enter the practical field, it is very probable that his success therein may be such that he could not be tempted back to teaching.

From the foundation of the Stevens Institute to the present time, the professors have been encouraged to do practical work. At all times this work has been done on their own responsibility, that is the institute was not responsible for any of the reports given out. On the other hand, it has been well appreciated that should erroneous or undignified reports be made it would reflect discredit on the institute, and great care has been taken that this should not occur. In 1894 the late Dr. Morton, who as you all know was the first president of the institute, established what he called the department of tests, the aims of which he described as follows:

> It is part of the institute's policy to make its laboratories and workshops the center of such experimental investigations as will be of direct commercial importance to the mechanical engineering profession, and likewise contribute to the same valuable technical information by which the knowledge of facts and principles which constitute the foundation of that profession may be enlarged. It is also part of the same policy to have its professors so in touch with the most advanced practise as to enable them to embody, in their courses of instruction, the best results of applied science in engineering practise.
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> A department of tests has, therefore, been organized to undertake measurements of the performance of steam-engines and other motors, and of the efficiency of boilers, refrigerating machines and mechanisms generally, including electrical and hydraulic apparatus, also to make tests of strength of materials, and to make various chemical and physical investigations, for the general public. Such work is assigned by the president to the member of the faculty best fitted to suc-

cessfully undertake any particular investigation, and the extensive facilities of the institute in the way of working space, apparatus, workshop appliances, skilled observers and mechanics are placed at the latter's disposal.

This might make it appear that all testing work from that time on was to be done by the institute as an institution, but such was not the case. No other than personal reports have been issued by the professors who have undertaken the work, and in every case the professors themselves have been entirely responsible for the payment of all expenses connected with the tests. In many cases the expenses of tests are quite large, and the payment of these must be secured either by obtaining a retainer or deposit from the parties for whom the work is to be done, or the professor making the test must run the risk of having to pay these expenses himself should the parties for whom he is doing the work fail to meet their obligations. This looking after the financial end of the problem is an essential one, as it gives the professors experience on the commercial as well as the engineering side of the work.

It has been claimed that the professors of an engineering college should not do work in the practical field, as this interferes with the consulting engineers who depend for their livelihood on just the sort of work that would be apt to be undertaken at a college. This is a very narrow view to take of the matter, and as far as my own personal experience is concerned, I can testify to the fact that much of the work undertaken in connection with my college duties has been done for consulting experts. The day is past when there can be a strict line drawn between the work of the consulting engineer and that of the professor who teaches in the same field. The ideal professor in a given line should be able to take up the work of the consulting engineer in that line, and the ideal consulting engineer should possess enough technical knowledge to fit him for being a professor. There should be no jealousy, but rather a bond of friendship in that the fundamentals which each should master are the same.

D. S. JACOBUS.
STEVENS INSTITUTE OF TECHNOLOGY.

ADDRESS BEFORE THE CENTRAL BOTANISTS.[1]

THE opening of the year 1902 was marked by the assembling in Chicago of the American Society of Naturalists, an association based on strict professional requirements for membership, which for reasons of expediency had limited its meetings to the eastern part of the country—a limitation specially set aside for the purpose of holding this Chicago meeting. With the Society of Naturalists had become affiliated a considerable number of equally strong professional organizations devoted to branches of nature study. All were largely indebted for their existence to the need that every student and teacher feels of the stimulus of personal contact with his peers in the work to which he is devoting his life.

The great summer gatherings of the American Association for the Advancement of Science, with its greater variety of interests and less strictly limited membership, had seemed not to give opportunity for this contact in the way desired, and the general and special bodies of naturalists, a large part of whom were also members of the association, had provided for meetings such as they desired in the short college recess of the Christmas season. Into this recess, lengthened for the purpose by a considerable number of colleges, the American Association had deliberately moved its own meeting, in the hope that the active workers of the entire country, in every field of science, might find it possible to meet together as a single great body, impos-

[1] Presidential address at the Ann Arbor meeting, December 28, 1905.

ing in its numerical strength, stimulating in its general program, and affording through adequate subject organization all of the advantages for which special and separate meetings had been arranged.

The Society of Naturalists itself had long exemplified a good model of combined organization by occupying on its own general program only such time as was actually required for the transaction of routine business, an evening when the president's address followed a dinner, and one afternoon devoted to a discussion of some subject of broad interest, by capable, well-informed speakers. All of the rest of the time was devoted to meetings of the affiliating special societies.

At the Chicago meeting the discussion turned upon the future of the American Society of Naturalists in view of the proposed invasion of its meeting time by the Association for the Advancement of Science. Having been invited by the executive committee to participate in the discussion, I confess that I went to Chicago undecided whether to urge the sacrifice of all that the separate meetings of the Naturalists seemed to stand for, or the transfer of these meetings into the summer season — vacated by the association. I have never attended so large a meeting at which conditions were so favorable for personal conference without the formation of cliques as at the Hotel del Prado, where every post-prandial cigar was the occasion of a general discussion participated in by the officers and other members of the association and the society, or of comparison of ideas with individuals representing every phase of interest involved. I had expected to find a feeling of irritation on the part of those most vitally interested in the society, at the *coup* which the association had effected, and was not unprepared to hear private suggestion that action should be advised adverse to meeting in connection with the association. On the contrary, whatever irritation may have been felt was kept out of sight, every one was disposed to try to realize the greater things that close union promised if effective, and my own mind clearly shaped itself into approval of the effort to secure for Convocation Week each year a great national meeting representative of American science as a whole. The public discussion did not show dissent from this conclusion.[2]

In deciding to try to meet with the Association for the Advancement of Science, the American Society of Naturalists did not for a moment contemplate passing out of existence or into desuetude. On the contrary, the decision led to its prompt reorganization on a national basis corresponding to its name, with provision for eastern, central and perhaps other branches, one or more of which should meet as the nucleus of the society in affiliation with the association, according to the place selected by the latter for its own meeting.

The second participant in the Chicago discussion began his remarks by quoting a resolution prepared for a business session, in which provision was proposed for the organization of a central branch of the American Society. My own opportunities for conversation with botanists of this part of the country had been many and favorable, and I felt that I presented their opinion also when I stated my belief that an organization of the botanists under such a central branch was desirable and probable.

Before adjournment, the Chicago meeting of the Society of Naturalists made tentative provision for the organization of a central branch, and as a member of the organization committee for this branch I am pleased to see the success that has attended the effort to provide for an annual naturalists' meeting within reach of every worker east of the Rocky Mountains. That

[2] Science, N. S., **15**: 241–255.

our western and southern colleagues may effect similar organization—with the like connection with the national society—is my earnest hope.

Since deciding for it, I have been an earnest and consistent advocate of the plan for holding the annual national meeting in affiliation with that of the American Association. The first effort to bring about this united action was made in connection with the Washington meeting of the association, a year after that of the naturalists in Chicago. Washington is a Mecca for every American. The interest of a visit to the national capital alone forms a great inducement to attend any meeting held there, and the attendance at this meeting was good and its program ample. Unfortunately, though, the meeting places were so scattered that practical difficulty was found in getting from place to place, and the program seems not to have been such as to give the concentration of special interest that the naturalists originally organized for, although their attendance at the meeting was large. I chanced to be closely connected with the provisions for the ensuing St. Louis meeting, and was able to assist the local committee in avoiding—so far as was in the power of such a committee—dispersal or clash of the many interests to be represented. Programs, however, are in the hands of the bodies that are to meet and not of a committee that is to provide conveniences for a meeting. The affiliating organizations were given dignified place on the general program of the association, their components were appropriately correlated, and the St. Louis committee had the satisfaction of winning commendation for a nearly ideal provision for the meeting, so far as its own duties went. A persistent gratuitous effort to adjust the meetings of sections and societies of like interests so that they should not clash was also made by the local committee, and met with the fullest cooperation of the officers of the association. These efforts resulted in very largely removing the difficulties that had been experienced in Washington, and led to the conviction that all clashes might have been removed if a little more care had been given to the preparation of special programs with reference to the work of the week as a whole. They also showed very clearly that far greater need exists than is generally and practically recognized, for the advance cooperation of the secretaries of all bodies that are to meet together.

The trial already made, however, had failed to convince the naturalists that the experiment of affiliation promised a full measure of success. The American Society of Naturalists—I thought at the time largely out of consideration for its president, who was charged with responsibility for the local arrangements for the St. Louis meeting—decided to meet again with the association; but several of its strongest component societies declined to follow the time-honored custom of adopting its meeting place as theirs, and the purpose of the naturalists not to meet in regular affiliation with the association seemed to be clear to a superficial observer, from what was overheard where men get together, and from their selection of a separate meeting place for this year, though I understand that a very good joint meeting was held at Philadelphia last winter, and one is hoped for at New York next year.

Thus it comes that the Botanists of the Central States have as presiding officer at this meeting a member of their executive committee and not the man whom they have honored by election as their president, who, called by conflicting duties at places as far apart as the Gulf and the Great Lakes, finds it possible to greet and thank his fellow members only through the voice of another—exemplifying in his own person the

present unfortunate decentralization of our national effort.

It should be clearly understood, though, that conflicting duties of this kind are of necessity to be expected, and that their occurrence is not inherently harmful. This is a proposition of easy demonstration, whatever may be the status of national scientific cooperation. The clearest note in the Chicago discussion that preceded our own organization was the pressing call for local societies.

It has been generally conceded that meetings at least once a year are vitally necessary for men having closely allied professional interests. It is equally conceded that our land is too large for all workers in a given field to flock to a single central point. However desirable it may be for us to have our interests represented in national societies that do meet together and that speak for us all in the way that unified action alone can make possible, it is evident that more of us must necessarily stay at or near home during Convocation Week than can possibly go to any general meeting place, even when this chances to be central —and the stimulus afforded to local growth by great national gatherings in every section is too valuable an agent in the furtherance of our purposes to be subordinated to the obvious immediate advantage of their restriction to easily accessible places. By going to the general meeting we shall always gain personal touch with men whom we never meet in any other way. But when we can not participate in the general meeting, we can get in the same personal touch with a smaller number of equally good men by attending the local meeting; and concentration of effort on our own specialties without the necessary distraction of conflicting attractions seems likely to yield us more real individual good at these local meetings. It is also possible that persons who go to remote national meetings may be given the privilege of attending those of a more local nature by the ultimate shifting of the latter into the short Easter recess, which, though too short for extended travel, may be found to suffice for this purpose.

We Botanists of the Central States may unaffectedly congratulate ourselves on the successful way in which our organization committee has performed its task. A charter membership of 124 men professionally trained in one science affords evidence of intelligent search for available timber. It affords equally gratifying evidence that botany between the Appalachians and the Rocky Mountains has enlisted the effort of men among whom mutual contact is to be now more possible than before; and in quality the list is as satisfactory as in numbers.

The mechanical trades have largely grown out of the journeyman system that once gave them a breadth that they no longer possess. In our professional life the migration of men from college to college is frequent and beneficial. Perhaps some of our charter members have already removed from our territory before the holding of our first regular meeting. If so, let us urge them to withdraw their names from our list and promptly add them to those of botanical organizations in their new homes —always cultivating the strongest possible federation of interests in national enterprise. Others have doubtless come within our range since the organization committee completed its work, and some already here may have escaped even the keen-eyed search of this committee. Can I close better than by expressing my earnest hope—which I am sure you all endorse—that all such, now and always, will promptly unite with us?

William Trelease.

THE AMERICAN MATHEMATICAL SOCIETY.

THE twelfth annual meeting of the American Mathematical Society was held at Columbia University on Thursday and Friday, December 28–29, 1905, simultaneously with the meetings of the American Physical Society and the Astronomical and Astrophysical Society of America. Large attendance and extensive programs characterized this three-fold gathering, in itself a noteworthy scientific occasion. Community of interest received due attention. A joint session of the Mathematical and Physical Societies was held on Friday afternoon for the purpose of hearing Professor V. F. Bjerknes, of the University of Stockholm, who spoke on 'Experimental Demonstration of Hydrodynamic Action at a Distance.' On Friday evening about ninety representatives of the three societies attended a dinner organized in honor of Professor Bjerknes. The common luncheon between each day's sessions afforded an excellent opportunity to renew and make acquaintance and to compare notes, scientific or otherwise. Informal gatherings were also held on Thursday evening.

The attendance at the four sessions of the Mathematical Society included sixty-six members, being a slight increase over the highest previous record. President W. F. Osgood occupied the chair, being relieved at the Friday afternoon session by Professor E. W. Brown. President Carl Barus, of the Physical Society, presided during the joint session. The following persons were elected to membership: Mr. R. L. Börger, University of Missouri; Professor W. B. Cairns, Ursinus College; Mr. A. J. Champreux, University of California; Dr. Emily Coddington, New York, N. Y.; Dr. F. J. Dohmen, University of Texas; Professor O. E. Glenn, Drury College; Mr. E. S. Haynes, University of Missouri; Professor J. H. Jeans, Princeton University; Mr. A. R. Maxson, Columbia University; Professor J. F. Travis, Georgia School of Technology; Professor Vito Volterra, University of Rome; Miss Mary E. G. Waddell, Orono, Canada. Nineteen applications for admission to membership were received. The total number of members is now 504, a net gain of 31 during the year 1905.

Reports were received from the treasurer, librarian and auditing committee. These reports will appear in the Annual Register of the society. The number of papers read at all meetings during the year was 147, as against 148 in 1904. The total attendance of members was 280; 161 members attended at least one meeting during the year. The library now contains about 2,300 bound volumes. The treasurer's report shows a balance of $3,833.01 on hand December 16, 1905; of this balance $2,132.58 is credited to the life membership fund.

At the annual election, which closed on Friday morning, the following officers and members of the council were chosen:

Vice-Presidents—Charlotte A. Scott and Irving Stringham.

Secretary—F. N. Cole.

Treasurer—W. S. Dennett.

Librarian—D. E. Smith.

Committee of Publication—F. N. Cole, Alexander Ziwet, D. E. Smith.

Members of the Council, to serve until December, 1908—C. L. Bouton, L. E. Dickson, Edward Kasner, E. J. Townsend.

The presidential term of Professor Osgood extends to December, 1906.

The following papers were read at the meeting:

R. G. D. RICHARDSON: 'Multiple Improper Integrals.'

A. B. FRIZELL: 'On the Continuum Problem (preliminary communication).

D. R. CURTISS: 'The Vanishing of the Wronskian and the Problem of Linear Dependence.'

J. I. HUTCHINSON: 'On Certain Automorphic Groups whose Coefficients are Integers in a Quadratic Field.'

E. V. Huntington: 'Note on the Fundamental Propositions of Algebra' (preliminary communication).

C. J. Keyser: 'Concerning a Self-reciprocal Plane Geometry.'

C. L. E. Moore: 'Geometry of Circles Orthogonal to a Given Sphere.'

Edward Kasner: 'Invariants of Differential Elements for Arbitrary Point Transformation.'

A. B. Frizell: 'A Method of Building up the Fundamental Operation Groups of Arithmetic.'

G. A. Bliss: 'A Proof of the Fundamental Theorem of Analysis Situs.'

O. P. Akers: 'On the Congruence of Axes in a Bundle of Linear Line Complexes.'

Peter Field: 'Note on Certain Groups of Transformations of the Plane into Itself.'

George Peirce: 'On a New Approximate Construction for π.'

Max Mason: 'Curves of Minimum Moment of Inertia.'

A. G. Webster: 'Application of a Definite Integral with Bessel's Functions to the Self-induction of a Solenoid.'

J. E. Wright: 'Correspondence and the Theory of Continuous Groups.'

J. E. Wright: 'An Application of the Differential Invariants of Space.'

Clara E. Smith: 'Abel's Theorem and its Application to the Development of an Arbitrary Function in Terms of Bessel's Functions.'

V. F. Bjerknes: 'Experimental Demonstration of Hydrodynamic Action at a Distance.'

R. P. Stephens: 'On the Pentadeltoid.'

M. I. Pupin: 'Establishment of a Steady State in a Sectional Wave Conductor.'

J. J. Quinn: 'A Linkage for the Kinematic Description of a Cissoid.'

The Chicago Section held its eighteenth regular meeting at the University of Chicago, December 29–30. The San Francisco Section will meet at Stanford University on February 24. The next regular meeting of the society will be held at Columbia University, February 24. The summer meeting, together with a colloquium, will be held at Yale University in August.

F. N. Cole,
Secretary.

SCIENTIFIC BOOKS.

Publications of the Jesup North Pacific Expedition. (Memoir of the American Museum of Natural History, New York.) Leiden, E. J. Brill, Ltd.; New York, G. E. Stechert & Co.

The continuation of the publications of the Jesup North Pacific Expedition appears, after an interval of several years, published by E. J. Brill, Leiden. The following notice of the publications issued during the year 1905 is written by the editor of the series, and for this reason contains only a brief statement of the contents of the volumes.

Vol. III., Part III., *Kwakiutl Texts.* By Franz Boas and George Hunt.

This number closes the volume containing the Kwakiutl texts recorded by George Hunt, and revised and edited by Franz Boas. The material in this volume has been arranged according to tribes of the Kwakiutl, beginning with the extreme south, and proceeding northward. The first text in the series is given in interlinear translation; while all the others are given in parallel columns, Indian and English. At the end of the volume is given a brief abstract of the traditions, which are intended to enable the reader to inform himself regarding the contents of the volume without reading the full texts. The abstracts are provided with page references, which facilitate the finding of any particular passage. An appendix to the volume contains lists of stem words and suffixes, by means of which the philological use of the text is facilitated.

The present volume contains almost entirely traditions relating to the ceremonies and families of the Kwakiutl Indians, and illustrates the exuberance of legends of this character that have developed among this tribe. In character and contents, these traditions are remarkably uniform. They resemble the traditions of the northern parts of the North Pacific coast, and account for the privileges of the different families and tribes of the Kwakiutl.

The language is probably, on the whole, accurate. Mr. Hunt, the recorder, speaks Kwakiutl as his own mother tongue, and has

been trained in writing the language by long practise, and under the guidance of the editor. All the texts have been phonetically revised with the assistance of other Indians. Notwithstanding the care that has been taken, there remain many uncertainties and obscure points; but the material seems sufficient to elucidate all the main points of the Kwakiutl language.

In the eighteenth chapter texts of speeches and war accounts are given, translations of which were published in Franz Boas's account of the 'Secret Societies and Social Organization of the Kwakiutl Indians,' published in the Report of the U. S. National Museum for 1895.

The style of most of the texts is diffuse, but it was thought well to retain the full accounts, because the stories contain a great many data relating to the every-day customs and beliefs of the tribe.

A second volume of texts of this tribe is in press. It contains the mythological traditions relating to the origin of the world, and supplements in this respect the material contained in the first volume.

Vol. V., Part I., *Contribution to the Ethnology of the Haida.* By JOHN R. SWANTON.

This volume contains parts of the results of an expedition undertaken by Dr. John R. Swanton to the Queen Charlotte Islands. His expedition was undertaken in cooperation with the Bureau of American Ethnology, the understanding being that the linguistic results (that is, the grammar and dictionary of the Haida language) were to be published by the Bureau of Ethnology, while the ethnological results and traditions were to be published by the Jesup North Pacific Expedition.

The present volume contains, primarily, data relating to the social organization of the Haida. In the first chapter of the book, interesting information is given on shamanism, witchcraft, medicine, customs, taboos and games of the tribe. In the beginning of the book the cosmic notions of the Haida are described, which are of great importance for a clear understanding of their social organization. It is interesting to note that the supernatural beings of the Haida are divided into two groups, in the same way as the tribes themselves—the Raven group and the Eagle group.

Perhaps the most important part of the author's discussion is the description of the division of the two sides of the Haida into families. The two sides, or clans, are exogamic, while the families are primarily local subdivisions of the clans. The detailed explanation of this grouping is given in a chapter entitled 'Haida History,' in which the author endeavors to present the history of the present families of the Haida as conceived by the Haida themselves. He begins with the mythological period, when the islands, the home of the Haida, arose from the ocean, and continues with the origin of the ancestors of the Raven clan and of the Eagle clan, through more or less mythical events, down to the historical events of the last few centuries, describing the gradual splitting-up and recombination of various families. Based on this discussion, he has reached the interesting conclusion, that, according to the idea of the Haida, the Raven clan is indigenous, while the Eagle clan may possibly represent descendants of immigrants from the mainland. There is, however, some evidence of a tendency to make the traditions of the two clans uniform.

The families settled in the various villages have certain prerogatives, the most important of which are the crests. A discussion of these shows that the principal crests of the Raven clan are the killer-whale and grizzly bear, while the principal ones of the Eagle clan are the eagle and beaver. Besides these, there are a great many scattering crests, many of which were obtained by purchase or gift, and which can not be in any way considered as totems.

A rather full discussion of the representation of the crest and of the myth in art contains detailed descriptions of a considerable series of totem-poles, showing that most of these are crest figures of a house-owner and of his wife, while others represent incidents in myths. Similar representations are found on grave-posts and on canoes, and on boxes, spoons and other utensils used by the people.

Several plates of tattooings representing crests are also discussed in this chapter, which is the most extensive explanation given, up to this time, of carvings and paintings from any one tribe of the North Pacific coast.

The description of the secret societies and potlatches of the Haida is not as complete as we should like to see it; but it is impossible at the present time to obtain full information on this point, because the old customs have become obsolete, and, owing to the great reduction in numbers of the tribe, the information which can be obtained now is fragmentary and contradictory. It is interesting, however, to note that the secret societies are also owned by various families, and that the conclusion previously reached of the introduction of the more important societies from the south is corroborated by information furnished by the Haida.

The last chapter of the book contains abstracts of Haida traditions. These consist of two series, one collected in Skidegate, another in Masset, and written in these two dialects of the Haida language. The Masset texts will be published in another volume of the publications of the Jesup Expedition. The Skidegate texts were written out by the author for publication by the Smithsonian Institution. A few texts and the translations of other traditions have just been issued as a Bulletin of the Bureau of American Ethnology. We may perhaps express the wish that a way may be found for publishing the full texts, which are required for a thorough study of the ethnology of the tribe. The abstracts of the traditions are accompanied by notes, giving parallel traditions from the North Pacific coast.

The volume closes with lists of the families, villages and houses of the Haida. This part of the book is accompanied by a number of interesting maps, compiled by Dr. Charles F. Newcombe, on which the native names of places and the locations of towns are recorded. These maps also contain many improvements on the last issue of the British Admiralty Maps.

Vol. VI., Part I., *The Koryak*. By Waldemar Jochelson.

Mr. Jochelson's description of the Koryak is based on his studies carried on in 1900–1 for the Jesup North Pacific Expedition. The subjects treated are the religion and the mythology of the tribe. The reason why the author began the publication of his studies with this subject was the necessity of coordinating his publication with that of Mr. Bogoras, who was at the same time publishing his studies of the material culture of the Chukchee. We obtain here for the first time an insight into the peculiar beliefs of the tribes of the Okhotsk Sea and of Kamchatka, which were first described by Steller.

The first chapter is taken up with historical remarks relating to previous information on the subject. In the second chapter a detailed description of supernatural beings is given. The principal of these is Big-Raven. He is looked upon by the Koryak as the founder of the world. He is also called Creator. In this respect, the Koryak belief differs from that of the Chukchee, who consider the Creator and Big-Raven as separate beings. Although Raven is the trickster of Koryak mythology, he is at the same time the great transformer, who has given the world its present shape. He is the first man, father and protector of the Koryak. Prayers are addressed to him, and he is appealed to in incantations. Sacrifices are also made to him. Almost all the Koryak myths, with very few exceptions, deal with the life, travels, adventures, and tricks of Big-Raven and his family.

Besides Big-Raven, the Koryak believe in a supreme being, the conception of whom, however, is vague. He sent Big-Raven down to our earth to establish order, and he seems to be the personification of the vital principle in nature taken in its entirety. He is described as an old man, living in a village in heaven, and having wife and children. Offerings are made to him to secure future prosperity, or as an atonement for the transgression of taboos. It is their belief, that, so long as the supreme being looks down upon earth, there is abundance and health, while, as soon as he turns away, disorder reigns. The supreme being does not seem to interfere in detail with the affairs of man.

A very important place in the system of religious ideas of the Koryak is taken by the kalau, or spirits, who appear as invisible beings. Sometimes they appear as common cannibals. These malevolent spirits are very numerous, and cause sickness and death. Some of them represent special diseases. The Koryak also believe in supernatural beings, that appear as rulers of various parts of the country, such as the 'master of the sea.'

As a protection against disease and misfortune sent by supernatural agencies, guardians and charms are used. The most important among these are the sacred implements for fire-making, which are considered the guardians of the reindeer herd, and to which are often attached rude carvings, representing the guardians' assistants. Among the Maritime Koryak, the fire-board is essentially the protector of the house. Carved wooden figures representing human beings are also used as guardians. Many of these are ornamented with sedge-grass. While many are small, there are also carved trees which stand near the house, and which are guardians of the house or of the village. Much valuable information relating to the significance of charms is given in this chapter.

The Koryak also used divining-stones, which are employed to divine the future by their movements when suspended from a thong. These are similar to the divining-stones of the Eskimo.

Mr. Jochelson discusses the method of shamanism from two aspects. There are professional shamans among the Koryak, who wear certain ornaments that distinguish them from other people, and who free the sick from disease inflicted by the evil spirits. The drums used by these shamans are similar in type to those used by other Siberian tribes. They differ from those of the Eskimo. Each family has also its own shamans, who protect the family. The peculiar ideas in relation to the change of sex of shamans which are found among the Eskimo, are also found among the Koryak.

Of especial interest is the description of the festivals and sacrifices of the tribe. The most important festival of the Maritime Koryak refers to whale-hunting, and consists principally of the welcoming of the captured whale, and of the ceremony accompanying its supposed return to the sea. In this festival masks made of wood and of grass are worn. The wooden masks resemble in type the simple masks of the northern Alaska Eskimo. The ceremonials of the Reindeer Koryak refer principally to the herd, and are intended to promote its welfare. A number of minor festivals relate to hunting.

The Koryak offer sacrifices to the supernatural beings. Both bloody and bloodless sacrifices occur. Among the former, the sacrifices of reindeer and of dogs are the most important. Mr. Jochelson describes in detail the peculiar custom of sacrificing dogs, and of attaching their bodies to poles or to the trees which represent the village guardians.

In the description of customs relating to burials, deaths and funerals, the complex burial customs deserve particular mention. The Koryak cremate the dead, who for this purpose are dressed in very elaborate costumes, which the people carry about during life, although they are finished only after death has occurred.

The whole second part of the book is taken up with the mythology of the Koryak, the material being arranged in geographical order. The whole mythology is remarkably uniform, dealing essentially with the marriages of the children of Big-Raven, and of his struggles with supernatural beings. Attention may be called, in this connection, to the brief characterization of Koryak tales given on p. 352 and the following pages. In the final chapter of his book, Mr. Jochelson gives a detailed comparison of the incidents found in Koryak mythology, with incidents of other mythologies of Siberia, of that of the Eskimo and of the North American Indians. It would seem that some of the elements contained in this comparison are so general, that perhaps their occurrence in these several mythologies may be without significance, so far as evidence of historical transference is concerned; but the results of Mr. Jochelson's statistical comparison are of considerable interest. He finds that among 122 episodes that belong to Kor-

yak mythology, 102 are also found in Indian myths, 30 in those of the Eskimo, and 25 in those of the Old World. He further finds that 8 are common to the Koryak, Indian, Eskimo and the Old World; 10 to Koryak, Indian and Eskimo; but none to the Koryak, Eskimo and Old World. From this the author draws the conclusion that the interchange of mythological elements between the Indians and the Koryak must be older than that between the Koryak and the Eskimo.

Vol. VII., Part I., *The Chukchee.* By Waldemar Bogoras.

The long-continued studies of Mr. Bogoras carried on in the Kolyma district from 1889 to 1898, and his later studies for the Jesup North Pacific Expedition at Anadyr and on the coast of the Chukchee Peninsula, enable him better than any one else to describe the ethnology of the Chukchee. His book is full of remarks which show the intimate acquaintance of the writer with the people he is describing. In the present volume, the habitat, the general characteristics and the trade of the people are described; but the principal contents of the volume relate to their material culture. The discussion of the methods of reindeer-breeding of the Chukchee leads to the conclusion that the domestication of the reindeer among them is probably recent; that in previous times the Chukchee were a littoral people, like the Eskimo, and that they lived principally by hunting sea-mammals. The method of treatment of the reindeer differs from that used by the Tungus and other western Siberian tribes. The domestication of the reindeer is less complete; it is not used for riding, but mainly for hauling sledges, and the method of harnessing is peculiar to the Chukchee. Mr. Bogoras also shows that the present method of dog-harnessing, which is the same as that used by other Siberian tribes, is probably a new one, and that formerly the dogs were harnessed in the same way as those of the Eskimo, *i. e.*, all attached at one point, not in pairs, as is customary at the present time. The various kinds of sledges used for the reindeer and the dog are also described in detail.

The method of hunting sea mammals is essentially identical with that used by the Alaskan Eskimo. In traveling on sea, the Chukchee use a skin boat, similar to the Eskimo boat. The kayak, with double-bladed paddle, is also used. Its distribution is rather peculiar. It exists on the Arctic Ocean, it is not found on the coast of the Pacific, but it appears again on the waters of the Middle Anadyr River. Then it disappears again for a long stretch, to appear finally on the Okhotsk Sea among the Maritime Koryak.

The traps are similar to those of the Eskimo, but a considerable amount of West Siberian influence may be noticed. Automatic bows and spring-traps, such as are found also among the Alaskan Eskimo, are clearly derived from Asiatic patterns. The throwing-board of the Eskimo occurs also among all the eastern Chukchee.

A detailed description of the sinew-backed bows and of the composite bow is given. The composite bows are similar in type to those found in more southern regions of Siberia. The throwing-whip (p. 158), which is used for propelling darts, is worth mentioning. Mr. Bogoras also describes the iron-work, which is used particularly for knives and lances. These are clearly influenced by the iron-work of the Yakut and of the Amur River tribes. Of especial interest is a description of armor, which was also formerly used by the Chukchee. This is made of small pieces of iron linked together and arranged in horizontal rows. The head was protected by a helmet of similar character, while around the neck there was a large wooden protector incased in hide, with movable wings. It seems probable that the ivory armor found in Alaska was an imitation of this iron armor, which, in its turn, may be related to the peculiar types of armor current in more southern parts of eastern Asia.

The detailed description of the Chukchee tent brings out the fact that the large and heavy tent of the tribe is not well adapted to the nomadic mode of life necessitated by the care of reindeer-herds. It seems plausible that the movable tent must be considered as a direct adaptation of the old permanent winter-house of the Maritime tribe to the

necessities of the present nomadic life of the tribe.

The clay lamps and kettles and other household utensils are similar to those of the Eskimo of the Yukon River.

The food of the Maritime Chukchee is to a very great extent derived from the sea, consisting largely of sea-mammals, while the Reindeer Chukchee live on reindeer taken from their herds. In connection with this subject, the author describes a number of taboos. Vegetable food is used rather as a substitute, in case of scarcity of meat, than as a side-dish.

In smoking, pipes evidently related to those of Chinese type are used.

One chapter of the book is devoted to a description of the manufactures, among which those relating to the preparation and utilization of skins occupy a prominent part.

The clothing is made of skins, that of the men consisting of skin boots and stockings, trousers and a double shirt, while the women wear combination-suits. It is peculiar to note that the fur jackets of the women are cut very low. In cold weather separate hoods are worn.

The women, particularly those of the Maritime Chukchee, are tattooed, and the tattooing is believed to have a magical significance. Many of the ornaments described by the author are also at the same time charms.

The book closes with a description of the games and sports of the people, among which tossing on blankets, wrestling and races play a prominent part. A number of ball games, and some cat's-cradles are described. The book is accompanied by many illustrations and by a detailed map, giving exact information as to the present location of the native tribes of northeastern Asia. It appears from this map that the Eskimo are confined to the region north of Anadyr Bay, and that the coast regions southwest of this district are occupied by the Kerek, a branch of the Koryak. Another map (p. 17) gives the approximate ancient distribution of the tribes before the invasion of the Yakut and of the Russians.

FRANZ BOAS.

Nebula to Man. By HENRY R. KNIPE. With fourteen full-page illustrations in color and fifty-seven full-page tinted illustrations by Ernest Bucknall, John Charlton, Joseph Smit, Lancelot Speed, Charles Whymper, Edward A. Wilson and Alice B. Woodward. London, J. M. Dent & Co. 1905. Small folio. Pp. xvi + 251.

This sumptuous volume, beautiful in typography, glowing with splendid illustrations from the studies of the most skillful delineators of animal life in the British metropolis, is a marvel in more ways than one. Its publication is remarkable from the standpoint both of the man of science and of the man of letters. It is 'an attempt to present a sketch of the evolution of the earth on the nebular hypothesis,' and this not in prose, but in the form of poetry. In six cantos the author traces the great drama of mundane evolution. The first division of the poem deals with the development of the globe from the nebula out of which it was evolved, and the beginning of the operations of life upon its slowly cooling surface; the next four cantos deal in order with the Paleozoic, the Mesozoic, the Cenozoic and the Quaternary ages; the last canto brings Neolithic man into view, and leaves us at the threshold of human history. The attempt to clothe the latest results of geological and paleontological research in the garb of poetry is daring. To marshal the facts of the paleontological laboratory in metric guise and to compel the sesquipedalian terms of the geologist and comparative anatomist to bend themselves to the service of the muse is bold indeed. While not always successful, nevertheless in the main the author has forced the cumbrous terms of science to do duty with grace, and has clothed a vast body of scientific facts in the garments of verse.

The opening lines, which face a splendid reproduction of a photograph of the great nebula in Orion made at the Yerkes Observatory, present a graphic picture of the planetary system in the making:

> A glowing mist, through realms of space unbounded,
> Whirls on its way, by starry hosts surrounded.
> Dim is its lustre as compared with theirs,

And more the look of stars dissolved it wears.
Volumes of heat, from its prodigious stores,
To endless space, it never ceasing pours.
Formless and void it seems, and yet it holds
A coming world within its hazy folds.
A sun lies spread within its depths and heights;
Planets are there, and all their satellites.
But still as yet they lie confused and blent,
Like starry dust, lost in the firmament.

A fine reproduction of the Spiral Nebula in Canes Venatici made at the Lick Observatory faces the second page of the book, and then, celestial parturition having taken place,

Loosed now are all the planets on their ways;
Each with its burden of collected haze
To run a destined course; and left alone
Spins the main core, a glowing central sun.
* * * * * * * * *
Here moves our planet, sun-held as the rest,—
Our mother Earth; but on her molten breast
No life as yet can dwell; and should the clouds,
Which gird her round with wind-swept vapour shroud,
Condense and pour their rains, no solid floor
Has she as yet on which to hold their store.

The birth of unicellular life, when at last the seas are formed, is told, and in rapid succession the development from age to age of the floras and faunas of the successive geologic periods is depicted. The mutations of the surface of the earth and the emergence and subsidence of the land masses, particularly those of Europe, are sketched, and the strange forms of vegetable and animal life are portrayed, as the great drama of development proceeds, issuing at last in the appearance of man upon the scene.

As an example of the manner of treatment employed by the author in delineating the main facts as to the animal life of the past, the following lines, culled from that part of the fourth canto which deals with the Jurassic age, may be quoted:

Great dinosaurs, like those of earlier days,
Still haunt Europa's woods and waterways;
And hold their own through all these Jura times,
In spite of lands wiped out, and changing climes.
* * * * * * * * *
Whilst through Europa's land these monsters range,
Upon Columbia's scenes are forms as strange.
Here lumbers Stegosaurus on his fours,
With high-arched back, a king of dinosaurs.
But forms surpassing Stegosaurs are seen.
In point of size, and of as wierd a mien.
Some here there are that look like plesiosaurs
With elephantine legs, as on all fours they creep along,

and reference is then made to 'knobbed Ceratosaurs,' 'necky Brontosaurs' and 'those long yards of life, Diplodoci.'

The illustrations of the book are particularly handsome, and represent the latest views of paleontologists of reputation. It would be invidious to draw comparisons, but the writer of these lines can not fail to express his pleasure at the rather spirited drawing by Miss Alice B. Woodward representing a *Diplodocus,* a beast with the bones of which the reviewer possesses considerable familiarity. Miss Woodward's sketches of *Mœritheria, Palæomastodon,* and *Arsinoitherium* are remarkably fine. There is great animation in her drawings, and she has profited to some extent, no doubt, by having at her elbow her father, one of the most honored and distinguished of living paleontologists, and his colleague Dr. C. W. Andrews, whose paleontological researches have given him a world-wide reputation. Very meritorious are also some of the drawings of Smit, which are based upon the work of the well-known American delineator, Charles R. Knight. The reproduction of the water-color sketch of 'A Frozen Sea' from the brush of Mr. E. A. Wilson, who has recently returned from the Antarctic voyage of the *Discovery* is appropriately inserted in that part of the poem which deals with the glacial epoch.

Upon the whole the book is most interesting and suggestive, and is one of the most entertaining contributions to popular literature dealing with paleontology and the doctrine of evolution which has recently appeared.

W. J. Holland.

SOCIETIES AND ACADEMIES.

NEW YORK STATE SCIENCE TEACHERS' ASSOCIATION.

The tenth annual meeting of this body was held at Syracuse, December 27–29. The officers for 1905 were:

President—A. P. Brigham, Colgate University.
Vice-President—G. M. Turner, Buffalo.
Secretary—J. E. Stannard, Owego.

The important matter of a biological survey of the state of New York was proposed in a paper by Professor Charles Wright Dodge, of the University of Rochester, and advocated by Director John M. Clarke, of Albany. Messrs. C. W. Dodge, of Rochester, C. W. Hargitt, of Syracuse University, and C. W. Hahn, of the Commercial High School, New York, were appointed a committee to further the project. The completion of ten years of fruitful work on the part of the association was made the subject of a special session, in which the four sections were represented by Professor William Hallock, of Columbia University, Professor A. D. Morrill, of Hamilton College, Principal C. T. McFarlane, of Brockport Normal School, and Professor E. D. Roe, of Syracuse University. Their addresses reviewed progress and looked into the future.

The section meetings were strongly sustained, and were largely given to practical discussions arising from the new regents' syllabus in the several departments. The association expressed itself favorably as regards the federation of the educational bodies of the state, provided its identity is fully preserved and it retains full liberty in all matters, particularly time and place of meeting. Ithaca was recommended to the incoming officers as the place for the next annual session. The association recorded itself as favoring legislation now pending in congress in regard to the adoption of the metric system, and joins with other bodies in certain recommendations concerning the teaching about narcotics and stimulants. The president for the coming year is Professor John F. Woodhull, of the Teachers College, Columbia University.

A. P. B.

THE SOCIETY FOR EXPERIMENTAL BIOLOGY AND MEDICINE.

THE fourteenth meeting of the Society for Experimental Biology and Medicine was held in the Rockefeller Institute, on Wednesday evening, December 20. The president, Edmund B. Wilson, was in the chair.

Members present—Adler, Atkinson, Auer, Beebe, Brooks, Burton-Opitz, Calkins, Crampton, Davenport, Dunham, Emerson, Ewing, Field, Flexner, Gibson, Gies, Hatcher, Jackson,[1] Levene, Levin, Lusk, A. R. Mandel, Meltzer, Morgan, Noguchi, Oertel, Opie, Pearce,[1] Salant, Shaffer, Wadsworth, Wallace, Wilson, Wolf, Wood.

Members elected—W. E. Castle, H. H. Donaldson, David L. Edsall, Thomas Flournoy, R. B. Gibson, Walter Jones, A. S. Loevenhart, John A. Mandel, Fritz Schwyzer, Frank P. Underhill, Francis C. Wood.

Abstracts of Reports of Original Investigations.[2]

The Action of Eosin upon Tetanus-toxin and in Tetanus, with Demonstrations: SIMON FLEXNER and HIDEYO NOGUCHI.

Eosin and certain other anilin dyes have the power of preventing in vitro the hemolytic activity of tetanus-toxin. Eosin destroys tetano-spasmin. Simultaneous injection of tetanus-toxin and eosin into rats delays or prevents the appearance of the symptoms of tetanus. When the symptoms appear they progress more slowly than in control animals.

The Action of Eosin and Erythrosin upon Snake Venom, with Demonstrations: HIDEYO NOGUCHI. (Communicated by Simon Flexner.)

The hemolytic principles of venom react differently to eosin, depending upon their native labilities. The hemolysin of *Crotalus* venom suffers most; that of *Daboia* next, while that of *Cobra* is most resistant. The toxicity of different venoms is more or less diminished by eosin in the light. Neurotoxin is little or not at all affected by eosin or erythrosin.

On Decomposition of Purin Bodies by Animal Tissues: P. A. LEVENE and W. A. BEATTY.

The authors have found that the presence of 0.5 per cent. of sodium carbonate in mix-

[1] Non-resident.

[2] The abstracts presented in this account of the proceedings have been greatly condensed from abstracts given to the Secretary by the authors themselves. The latter abstracts of the communications may be found in current numbers of *American Medicine* and *Medical News*.

tures of spleen pulp facilitates the decomposition of purin bodies to such an extent that even uric acid is broken up by that tissue. The products were non-basic in nature.

On the Biological Relationship of Nucleoproteid, Amyloid and Mucoid: P. A. Levene and John A. Mandel.

The authors have subjected nucleoproteids to decomposition in acids and alkalis. Among the products thus obtained were substances resembling glycothionic acids. This observation indicates that nucleoproteids, amyloids and mucoids are genetically related.

Imperfection of Mendelian Dominance in Poultry Hybrids, with Demonstrations by Photographs and Plumage Charts: C. B. Davenport.

The author has observed that in poultry hybrids the dominant character is frequently modified by the presence of the recessive and in the direction of the latter. For example, white plumage color may dominate over black, but the white hybrid shows some black feathers; white dominates over buff plumage, but the hybrids have a buff cast. When the hybrids are interbred the recessive character reappears in about one fourth of the hybrids, but often so modified as to be scarcely recognizable. The fact of the mutual contamination of characters in hybrids justifies the warnings given by breeders as to loss of characters in hybridization and the care that they exercise to maintain pure races.

The Mechanism of Conduction and Coordination in the Heart, with Special Reference to the Heart of Limulus: A. J. Carlson. (Presented by Russell Burton-Opitz.)

The author has shown that in the heart of *Limulus* the rhythm is neurogenic, not myogenic, and that the conduction and coordination take place in the nervous and not in the muscular tissue. The rate of conduction in the intrinsic heart nerves of this animal, as found by the author, is 40 cm. per second and the rate in the motor nerves to the limbs is 325 to 350 cm. per second.

Further Observations on the Effects of Alcohol on the Secretion of Bile: William Salant.

In continuation of his studies on this subject the author has found that intravenous injection of alcohol causes diminished flow of bile. Introduction of alcohol into the gastro-intestinal canal is almost always followed by increased flow of bile. Further experiments are in progress to determine the explanation of these facts.

Some Effects on Rabbits of Intravenous Injections of Nicotin, with Demonstrations: I. Adler and O. Hensel.

After eighteen daily injections of 1.5 milligrams of nicotin slight changes are apparent in the bulb and arch of the aorta. After thirty-eight daily injections very marked and characteristic macroscopic and microscopic lesions can be recognized. Aneurysmatic dilatations of the aorta are very distinctly visible. There may be either a single aneurysm, or, as is more frequently the case, several in various parts of the aorta. These dilatations as a rule do not involve the entire circumference of the vessel, but only a limited portion, thus presenting the appearance of aneurysmatic pouches. The more frequently the injections are repeated daily the more pronounced and extensive the alterations appear, but they are always of the same character. The lesions here referred to have nothing in common with human arteriosclerosis. The work is still in progress.

Tumors of Wild Animals Living under Natural Conditions: Harlow Brooks.

The author referred to the great importance of the etiology of neoplasms and the well-recognized fact that research along this line must now rest almost entirely on experimental studies of the lower animals. Of 2,645 living animals which have come under the observation of the author at the New York Zoological Park during the past five years, no case of true neoplasm has been found. 744 animals have died and, as is the routine custom at the New York Zoological Park, have been autopsied, either by the resident pathologist or by the author. In this series of 744 consecutive cases but one case of tumor has been found (white raccoon dog; myxo-sarcoma of the ovary). Tumors of parasitic origin, granulomata, tubercles, actinomycotic foci and

the like are, on the other hand, relatively common. Numerous examinations of various animals taken in the wild were made by the author, with the same negative result. Abnormal conditions of life unquestionably increase the relative occurrence of new growths.

The Cutaneous Excretion of Nitrogenous Material: F. G. BENEDICT. (Presented by William J. Gies.)

The author found that the average amount of nitrogen that was eliminated through the skin daily by healthy men at rest was 0.071 gram. At hard labor healthy men eliminated through the skin as much as 0.22 gram of nitrogen. The exact nature of the compounds in which the nitrogen was eliminated has not yet been ascertained, but the author thinks it highly probable that urea and ammonium compounds are the leading products. The author alluded to the great significance of these observations in any study of the metabolism of protein, especially in experiments in which the total amounts of nitrogen in the ingesta and egesta are smaller than normal, since the percentage error is thereby proportionally increased.

The Effects of Intravenous Injections of Solutions of Dextrose upon the Viscosity of the Blood: RUSSELL BURTON-OPITZ.

When small quantities (5 c.c.) of a concentrated solution of dextrose were injected intravenously, the viscosity of the blood became slightly greater. By the administration of large quantities (50–100 c.c.) the viscosity was markedly decreased at first, but reassumed its normal value in the course of about one hour. By producing artificial glycosuria, the viscosity was decidedly increased. In the latter series of experiments the surface of the pancreas was painted with solution of adrenalin. The specific gravity of the blood pursued in all cases a harmonious course with the viscosity.

WILLIAM J. GIES,
Secretary.

THE SOCIETY OF GEOHYDROLOGISTS, WASHINGTON.

THE first regular meeting of the society was held on December 20.

After the adoption of a constitution, Mr. G. B. Richardson, to whose initiative the organization of the society was due, was elected president and Mr. M. L. Fuller, secretary.

Mr. Fuller spoke on "The Use of the Term 'Artesian' as Applied to Wells in the United States." From circular letters sent out to geologists, geohydrologists and others working on artesian problems a table was compiled showing preferences as to the use of the term. It was recognized in nearly all the replies that the original application to flowing wells was on general grounds desirable, but in view of the difficulties of such an application in field work there was considerable doubt expressed as to the desirability of so restricting its use. The replies were summarized as follows:

From—	Favoring Retention.	Favoring Abandoning.
Field men	23	77
Men combining field and teaching experience	50	50
Teachers with limited field experience	71	29

The feeling in favor of retaining the original definition was in a general way inversely proportional to the amount of field experience, those dealing with field problems in most instances preferring a modified definition. The general sentiment was favorable to applying the term to all wells in which the water is under hydrostatic pressure, regardless of whether they flow or not. Some favored the dropping of the term altogether because of the indefiniteness of its present use.

Mr. C. A. Fisher spoke on 'A New River in Northern Nebraska.' Water first began to appear early last summer in low spots along a broad shallow valley-like depression in Cherry County, gradually increasing in amount until a stream was formed, which during the summer slowly pushed its way, it is reported, forward across Brown, Rock, Holt and portions of Wheeler and Antelope Counties, a distance of about one hundred miles. There is said to be no record of any water course in the valley since the region was settled and numerous more or less fantastic explanations have been advanced by the inhabitants to explain its appearance. In reality, it probably simply represents the surplus groundwater which has been accumulating during

several seasons of unusual rains, and the river, therefore, simply marks a rise of the ground-water level to a point above that of the valley bottom. It is believed that it will disappear when the ground-water is again reduced to its normal level.

M. L. Fuller,
Secretary.

UNIVERSITY OF COLORADO SCIENTIFIC SOCIETY.

During November and December, 1905, the society held seven meetings. The papers presented were as follows:

Professor M. F. Libby: 'Growth in Childhood and Adolescence.'

Dr. E. Barber Queal: 'The Causes of Dyspepsia.'

Professor Charles C. Ayer: 'The Phonograph in Modern Language Teaching.'

Dr. William P. Harlow: 'The Blood in Health and Disease.'

Judge Junius Henderson: 'Extinct and Existing Glaciers of Colorado.'

Dr. Luman M. Giffin: 'The Necessity for Pure Water.'

Professor M. S. Ketchum: 'Sources of Water-Supplies and Methods of Distribution.'

Dr. Dessie B. Robertson: 'Methods of Bacteriological Analysis of Water.'

Dr. George H. Cattermole: 'The Pollution of Water-Supplies.'

Professor Francis Ramaley: 'Noted Typhoid Epidemics.'

Two evenings were given to the papers devoted to the subject of water supplies. An attempt is being made by the society to inform the public in regard to proper means of securing good water. At a city election held in Boulder shortly after these meetings the vote was overwhelmingly in favor of the extension of the water-works. It is thought that the influence of the society was considerable in bringing about this result.

Francis Ramaley,
Secretary.

Boulder, Colo.,
December 22, 1905.

DISCUSSION AND CORRESPONDENCE.

MENDELIAN INHERITANCE AND THE PURITY OF THE GAMETES.

The communication on the above subject by my friend and colleague Professor Morgan, printed in the issue of Science for December 29, offers an ingenious if somewhat complicated interpretation of Mendelian inheritance that is at variance with the current conception in that it substitutes for a disjunction of allelomorphic characters in the gamete-formation a reversal of dominance in half the gametes (which evidently involves some kind of disjunction of the factors that determine dominance). That a complete elimination of the dominant character does not take place in the production of albinos (this character still being present in what Castle has called the 'latent' state) has been clearly recognized by several experimenters and is beautifully demonstrated by Cuénot's work on mice; but Professor Morgan's attempt to find a general basis for the explanation of this is a new and interesting contribution to the subject. I think, however, that his effort to explain the very case (that of Cuénot's yellow mice) that suggested his new interpretation, involves a negation of Cuénot's experimental results. This observer found that yellow was invariably dominant to all other colors, but that after crossing yellow mice with pure-bred grays (or other colors) the yellow mice of F_2, contrary to his expectation, included no pure extracted dominants, nor could such a race be obtained from them. In order to explain this, Cuénot advances the hypothesis that the yellow-bearing gametes are sterile to one another or do not unite, *i. e.*, a selective fertilization occurs, such that the yellow-bearing gametes are fertilized only by those bearing other colors. The interest of the question in relation to sex-production (which I have discussed in a paper now in course of publication) leads me to offer a word of criticism, since I am unable to share Professor Morgan's belief that his assumption will take the place of Cuénot's hypothesis. This belief rests, I think, on a misconception of Cuénot's results regarding the behavior of the yellow mice of F_2 which possibly arose through a confusion of Cuénot's formulas with his statement of fact.

How was the constitution of these mice tested? As in all similar cases, by the nature of their offspring, and Cuénot clearly specifies two methods by which the test was applied.

namely, (1) by pairing the yellow mice with each other, and (2) by crossing them back with pure grays, blacks or browns. In either case, pure or homozygous F_2 yellow mice (or in Professor Morgan's view those that contain only 'latent' as opposed to 'free' gray) should give only yellow offspring, owing to the uniform dominance of yellow, while mixed or heterozygous yellows (yellow mixed with 'free' gray) should produce grays or other colors as well as yellows. Cuénot says: 'J'ai essayé par l'une et l'autre méthodes un nombre considérable (81) de Souris jaunes * * *.' 'Or, a mon grand étonnement, je n'en ai pas trouvé une seule (*i. e.,* homozygote).' Professor Morgan considers this statement, which embodies the principal result of Cuénot's experiments, as 'somewhat ambiguous,' apparently for the reason that Cuénot does not in this passage actually use the words that all the yellow mice produce offspring of other colors as well as yellows; but it must be obvious that only such a result could justify his statement, and if this be not Cuénot's meaning I am unable to discover any meaning in his paper. In point of fact, however, he states specifically on a preceding page (cxxvii) that the cross between a yellow mouse and a pure-bred one of a different color always gives offspring of this color (gray, black or brown) in addition to yellows, the numbers being stated, in the case of the yellow-gray cross, to be equal.

Now, according to Professor Morgan's assumption there should on his own showing be two classes of yellow mice in F_2, of which "the first group $CY(CG)$ (*i. e.,* those containing 'latent' gray) will breed true, the other group $CY(CG)(CY)CG$ (containing 'free' gray) will split up in each successive generation according to the Mendelian formula." Such a behavior of the F_2 yellow mice was precisely Cuénot's expectation, but 'to his great astonishment' it was contradicted by the results. Professor Morgan, nevertheless, insists that the case of the yellow mice is precisely similar to that of extracted gray dominants (both being 'contaminated' by the recessive character in the latent condition) though, as Cuénot was the first to show, the latter breed true save for the rare appearance of a different color, such as black, probably derived from the latent color of the original albino used. If the two cases do not differ, why was so experienced an observer as Cuénot astonished at his results, and why did he go so far out of his way to construct the special hypothesis of selective fertilization to explain the behavior of the yellow mice as distinguished from those of other colors?

The difficulty with Professor Morgan's explanation is that it proves too much, for it explains the special peculiarities of the yellow mice out of existence (!). My criticism is not directed against Professor Morgan's general assumption, but I think that it entirely fails, as far as he develops it, to account for the peculiarities of the yellow mice, and that it leaves Cuénot's hypothesis, which it is supposed to obviate, exactly where it stood before.

E. B. WILSON.

THE LOGICAL BASIS OF THE SANITARY POLICY OF MOSQUITO REDUCTION.

THE excellent address of Sir Ronald Ross under the above title published in the December 1 number of SCIENCE, states the general rules regarding mosquito distribution with great accuracy; but it applies only to certain species, including the *Anopheles* so far as known to me and to *Stegomyia fasciata.* It does not apply in the least to such forms as *Culex cantator* and *C. sollicitans.* None of the suggestions as to erratic flights that practically restrict the distance traveled influence these species, which are truly migratory and are guided by some motive other than finding food or a place to breed. In fact, as I have shown, these migrants never propagate their kind and where they are to be dealt with, all of the carefully reasoned mathematical deductions fall. The matter is of great practical importance in New Jersey where communities within whose boundaries not a mosquito breeds, nevertheless, sometimes find life a burden because of the insects. Local work in such cases is worse than useless. When we find the dominant mosquitoes in the Orange Mountains to be species whose nearest breeding place is on Staten Island, time and money

spent in the Oranges would be obviously wasted.

As applied to the usual inland species the argument made is fully borne out by my field experience. As to the salt marsh breeders it is utterly inapplicable—witness the fact that the work done on the Newark meadows resulted in a marked decrease in the mosquito troubles at Paterson many miles to the north.

John B. Smith.

New Brunswick, N. J.,
December 15, 1905.

YELLOW FEVER AND THE PANAMA CANAL.

To the Editor of Science: The continuous discussion of Panama Canal affairs suggests to me to call attention to the possibility that the cutting of the canal may lead to trouble from yellow fever in two of our Pacific island colonies. In the summer of 1902, spent in the Hawaiian and Samoan islands as agent of the U. S. Bureau of Fisheries, my attention was forcibly called to the unusual proportions of the mosquito plague in both these island groups. If it were not for the dragonflies which wage effective war against the 'day mosquitoes,' and for the bed canopies of netting which protect the sleeper from 'night mosquitoes,' life would hardly be tolerable in Honolulu. In Tutuila (our principal Samoan island) mosquitoes are the most obvious features of the above-water fauna aside from the brown natives themselves. Now both in Hawaii and Samoa one of the most abundant of the infesting mosquito species is *Stegomyia fasciata,* which is none other than the yellow-fever mosquito, that is, the particular mosquito species which harbors and disseminates, in yellow fever regions, the plasmodium or bacterium which is the immediate cause of the disease.

So far no cases of yellow fever have occurred in Hawaii or Samoa, but this is obviously not because of the absence of the yellow fever host, but, presumably, of the yellow fever specific causal agent, the pathogenic 'germ.' It is to be presumed that ships have not yet carried yellow-fever-germ-infested specimens of *Stegomyia* from the West Indies to Hawaii or Samoa. Going round the Horn is probably an effective check to the spread of yellow fever from the West Indies to our Pacific Islands by reason both of the time required and the low temperatures met. Besides there is little traffic now between the two regions. But with the cutting of the canal, making possible a direct short-time passage of ships from the Gulf of Mexico to Hawaii, or to Samoa, all of the voyage being within tropical or subtropical latitudes—the Hawaiian islands are in 20° north latitude, the Samoan islands in 14° south latitude—will there not be a real danger of planting the dread agent of yellow fever in our Pacific colonies in which already the necessary insect host exists in enormous numbers? There may be obvious reasons why this migration can not take place, but they are not apparent to me now. It is, at least, a contingency to be had in mind by those charged with the responsibility of public health affairs in Hawaii and Samoa.

Vernon L. Kellogg.

Stanford University, Calif.

REPORT OF THE TENTH GEOLOGICAL EXPEDITION OF HON. CHARLES H. MORRILL. SEASON OF 1905.

The season of 1905 marked a renewal of paleontological activity in the University of Nebraska, since it so happened that for the first time in several years funds became available again for the prosecution of such work.

By virtue of the liberal support and patronage of Hon. Charles H. Morrill, of Lincoln, annual geological expeditions, essentially paleontological in character, had been maintained in connection with the state university since 1892. In 1901, though his interest in the work as well as his good will continued, his patronage ceased. This was wholly due to the overcrowded condition of the state museum, coupled with unusual fire risks, which plainly endangered public and private collections. In the meantime the work of making general collections has been pushed by the state survey, but the special work conducted by the annual Morrill geological expeditions was necessarily of a desultory order, the expenses being met by the sale of duplicate specimens.

Pursuant to recommendations by Chancellor Andrews and the board of regents, the state legislature early in 1905 voted the sum of fifty thousand dollars for the erection of a portion of the first wing of a fireproof museum. With the assurance of safe and ample room and increased facilities Mr. Morrill again offered substantial support to the amount of one thousand dollars annually for paleontological research and exploration.

This is an important sum, especially to a young institution and to those living near the fossil fields where student labor is to be depended upon, and where through friendly interest in scientific investigation the railroads of the commonwealth stand ready to extend to the university free transportation and other courtesies.

Early in the summer a small party of students was organized, and, in response to invitations from Mr. James Cook, of Agate, Nebr., camped and collected on his extensive ranch, which includes some twelve miles of Loup Fork exposures along the Niobrara River. The season was spent at one spot where, in a thin layer, the bones occurred in such numbers that they were literally quarried. As heretofore the Burlington and Missouri River Railroad offered free transportation for men and material.

Personnel of the tenth expedition: L. J. Pepperberg, H. J. Cook, M. L. Lee, J. H. Miller, W. D. Steckelberg and the writer, who was in charge.

Field work in Nebraska is not necessarily confined to summer, for fall is a protracted and open season, and many excursions are yet to be made before the year ends. Collections of the economic resources of the state at large are being made by Dr. George E. Condra. Special collections of the economic resources and fossils of Sarpy County are being made by Mr. L. J. Pepperberg, and the work of collecting is now being extended to eastern and southern fields by Mr. Charles N. Gould, professor of geology in the University of Oklahoma, while pursuing courses of study leading to his doctorate.

Plans for a new state museum are drawn and approved, and it is promised that the first portion of a fire-proof wing will be ready for occupancy within a year.

This, coupled with the fact that funds are available from several sources, brightens the outlook for geological and paleontological work in the University of Nebraska, where for the past year more than one ton a week of the best state collections have been boxed and lowered in an abandoned steam tunnel under the campus.

An account of the Morrill Geological Expeditions 1892 to 1900 by Miss Carrie Adeline Barbour may be found in SCIENCE, Vol. XI., No. 283, pages 856–858, entitled 'Report on the Work of the Morrill Geological Expeditions of the University of Nebraska.' An account of these expeditions may also be found in Vol. I., pages 18–24, of the Nebraska Geological Survey, under the title 'History of the Morrill Geological Expeditions.'

ERWIN HINCKLEY BARBOUR.
THE UNIVERSITY OF NEBRASKA,
LINCOLN, NEBRASKA,
November 1, 1905.

REPORT TO THE TRUSTEES OF THE ELIZABETH THOMPSON SCIENCE FUND OF PROFESSOR BOVERI'S RESEARCHES.

THE following report has been received from Professor Boveri and is now published by order of the trustees:

I herewith permit myself to make report concerning the investigations which I have carried out with the support of the Elizabeth Thompson Science Fund. I spent seven weeks at the Zoological Station in Naples, where I occupied myself, in connection with earlier experiments, on the development of dispermic sea urchin eggs with the following questions:

1. It is of fundamental importance for the whole problem of dispermy to determine whether dispermic germs develop pathologically because they have taken in two spermatozoa or because they were already pathological. I have, therefore, tested this question experimentally. One of the experiments succeeded in every respect so perfectly that the assertion can now be made with complete certainty that the same egg which, if impregnated by a single spermatozoon would have

developed normally, will, if impregnated by two spermatozoa, develop pathologically.

2. As I have previously stated,[1] the isolated blastomeres of dispermic eggs differ extremely in their developmental potency; but it was formerly not possible for me to follow the single blastomeres in their development so as to be able to assert with complete positiveness that there had not been during the isolation an unequal degree of injury to the blastomeres which might have been the cause of the inequality of the development. This I have succeeded in accomplishing in my new experiments. It has been possible to show that the early development up to the blastula stage proceeds identically in the isolated blastomeres of a dispermic germ, and that only later does one partial germ strike out in one direction, the other in another direction.

3. In connection with this I have studied thoroughly the early development of uninjured dispermic germs and they confirmed the corresponding original similarity and later unlikeness of the single germ areas.

4. A further question concerning the development of dispermic germs was whether the so-called primary mesenchyme cells which later group themselves to form the regular mesenchyme crown for the formation of the calcareous skeleton always occur only in that area of the germ in which they arise, or whether the mesenchyme represents an indifferent material, the cells of which are distributed by accident to the crown of mesenchyme. By means of the difference in size of the cells and nuclei in dispermic germs, it became possible for me to decide this important question in favor of the last alternative.

5. Against the theory which I formerly advanced upon the basis of my experiments on dispermic development of the different valence of chromosomes, the objection might be raised that it was not, as I had suggested, the false combination, but the incorrect number of the chromosomes which was of pathological significance, for it might be said that only when the requisite quantity of chromatin is present that the proper relation of nucleus and protoplasm can exist which is necessary to the normal functioning of the cell. In order to exclude entirely this objection it must be shown that for any given quantity relation of nucleus and protoplasm in the starting cell, the proper relation of the two constituents can be reached in the larva cell. This could be proven by the rearing of fragments of eggs of all sizes in which, therefore, the amount of the nuclear material remaining constant various amounts of protoplasm were present. These experiments were so carried out that the egg fragments were reared in quantities and in these cultures every thinkable size of normal larvæ was found. Again, various sized egg fragments were measured accurately and reared isolated, and these also developed into normal larvæ of all sizes.

6. The important task which I had set myself was the following of dispermic eggs in which only one sperm nucleus united with the egg nucleus while the other remained independent. I succeeded in rearing twenty-two specimens in which this rare and theoretically especially important type of dispermy occurred. From these I obtained a considerable number of gastrulæ and plutei. These larvæ consist of one part with large nuclei and one with small nuclei, and by this mark it may be determined with complete certainty what part contains paternal nuclear substance and what part contains paternal and maternal nuclear substance combined.

The various experiments enumerated will enable me to finish my work on double impregnation, and I hope that the completed memoir will be published in the spring of 1906.

Charles S. Minot,
Secretary.

[1] 'Über mehr polige Mitosen als Mittel zur Analyse des Zellkerns,' *Verh. d. phys.-med. Ges. Würzburg*, 1902.

THE CONGRESS OF THE UNITED STATES.

On January 6 a bill was introduced by Mr. Kahn, to provide for celebrating the four hundredth anniversary of the discovery of the Pacific Ocean by Vasco Nunez Balboa by holding an international exhibition of arts, industries, manufactures, and products of the soil, mines, forest and sea, at the city of San

Francisco, California; referred to the Committee on Industrial Arts and Expositions.

On January 8 Senator Smoot introduced a bill for the protection of wild animals in the Grand Canyon Forest Reserve; referred to Committee on Forest Reservations and Protection of Game.

On January 9 Representative Lacey introduced a bill for the preservation of American antiquities; referred to the Committee on Public Lands.

On January 10 Senator Perkins introduced a bill for the protection of animals, birds and fish in the forest reserves; referred to the Committee on Forest Reservations and the Protection of Game.

The bill for the incorporation of the American National Institute at Paris has passed the senate.

SCIENTIFIC NOTES AND NEWS.

DR. WILLIAM RAINEY HARPER, president of the University of Chicago, died on January 10, at the age of forty-nine years. At the funeral exercises at Chicago, on January 14, addresses were made by President Faunce, of Brown University; Chancellor Andrews, of the University of Nebraska; Dean Judson, of the University of Chicago, and Dr. Lyman Abbott, of New York City. It is intended to bury the body ultimately in a memorial chapel and crypt to be built on the university grounds. Memorial exercises were also held at Columbia University, addresses being made by President Butler, President Wilson, of Princeton University, and President Hall, of the Union Theological Seminary.

PROFESSOR J. P. IDDINGS, of the University of Chicago, was elected president of the Geological Society of America at the recent Ottawa meeting.

DR. N. L. BRITTON, director of the New York Botanical Garden, has been elected president of the New York Academy of Sciences.

DR. A. L. KROEBER, of the University of California, has been elected president of the American Folk-lore Society.

PROFESSOR HENRY M. HOWE, head of the department of metallurgy in Columbia University, has been elected a foreign member of the Swedish Royal Academy of Sciences. The other Americans holding this honor are Professor Simon Newcomb, Dr. Samuel Pierpont Langley, President Charles R. van Hise, Dr. Alexander Agassiz and Mr. Thomas A. Edison.

DR. BURTON E. LIVINGSTON has resigned his position in charge of the Division of Soil Fertility of the Bureau of Soils, U. S. Department of Agriculture, to accept a place on the staff of the Desert Botanical Laboratory of the Carnegie Institution, at Tucson, Ariz.

THE resignation of Dr. D. T. MacDougal as assistant director has brought about a reorganization of work at the New York Botanical Garden. Dr. W. A. Murrill, who has been serving as a curator for parts of two years, having succeeded to the position left vacant by the resignation of Professor F. S. Earle to accept the work of directing the Cuban Agricultural Experiment Station, has been appointed first assistant, and the duties of Mr. Percy Wilson, administrative assistant, have been increased. Dr. C. S. Gager, who has pursued investigations at the garden for some time under the direction of Dr. MacDougal, has been appointed director of the laboratories. Mr. R. S. Williams, who has done much field work on behalf of the garden during the past five years in the Yukon Territory, Bolivia and the Philippine Islands, has been appointed an assistant curator. Mr. C. B. Robinson, who has been a student of the garden, giving special attention to the study of the stone-worts, and to the Philippine Island collections formed by Mr. Williams, has also been appointed an assistant curator.

AT the annual election of the American Philosophical Society, held on January 5, the following officers were elected for the ensuing year:

President—Edgar F. Smith.

Vice-Presidents—George F. Barker, William B. Scott, Simon Newcomb.

Secretaries—I. Minis Hays, Edwin G. Conklin, Arthur W. Goodspeed, Morris Jastrow, Jr.

Treasurer—Henry La Barre Jayne.

Curators—Charles L. Doolittle, William P. Wilson, Albert H. Smyth.

Councillors—Patterson Du Bois, Samuel Dickson, Ernest W. Brown, William Keith Brooks.

Dr. William James, professor of philosophy, at Harvard University, is at present lecturing at Stanford University, where he will stay until June.

The fifth lecture in the Harvey Society Course will be given by Professor W. H. Park, at the New York Academy of Medicine, on Saturday, January 20, at 8:30 P.M., his subject being 'A Critical Study of Serum Therapy.' All interested are invited to attend.

Dr. E. Pflüger, professor of physiology, at Bonn, celebrated, on December 15 the fiftieth anniversary of his doctorate.

Dr. Max Heinze, professor of philosophy, Leipzig, celebrated, on December 13, his seventieth birthday.

Sir Michael Foster and Sir Philip Magnus are candidates for parliament from the University of London.

A committee has been formed under the patronage of Prince Bernhard of Saxe-Meiningen for the erection of a memorial of the late Professor von Mikulicz, at Breslau.

A position as computer at the Yerkes Observatory is now vacant. The incumbent, who may be either man or woman, will devote considerable time to measuring and reducing stellar spectrograms, in addition to performing miscellaneous computations. The salary which can be offered at present is fifty dollars per month. Applications should be made to Professor Edwin B. Frost, Williams Bay, Wisconsin.

The French Association for the Advancement of Science will meet at Lyons, on August 2, under the presidency of M. Lippmann, professor of physics in the Sorbonne.

Mr. W. D. D. Crotch has left his residuary estate (some £8,000) to the Museum of Zoology, Oxford University. His brother, Mr. G. R. Crotch, had previously left considerable gifts in collections, books and money, to the same institution.

The department of entomology of the American Museum of Natural History has received as a gift from William Schaus, Esq., formerly of New York City, a valuable collection of moths embracing some 26,000 specimens, mainly from Mexico, Central America and South America. Four years ago the museum received from the same gentleman a collection of 5,000 butterflies, including many rare specimens from Europe, Asia, Africa, Australia and New Zealand.

Señor Teodoro Dehesa, the governor of Vera Cruz, Mexico, has donated to the Carnegie Museum, Pittsburg, Pa., a Mexican idol, which was regarded as the gem of his archeological collection. The idol was stolen some years ago and finally came into the possession of the Carnegie Museum, which purchased it, without knowing its previous history. When the authorities of the museum discovered the facts, they offered to restore the specimen to its owner, but he has requested them to retain it as his gift.

It is stated in the London *Times* that Mr. W. O. B. Macdonough, of San Francisco, California, has presented to the trustees of the British Museum the skull and limb bones of the celebrated race horse Ormonde, in the opinion of many good judges the best horse of the nineteenth century. The remains recently arrived in good condition, and are now in the hands of the museum preparator to be mounted for exhibition in the hall of domesticated animals, which already contains specimens of several English thoroughbreds, including the skull of Bend Or, the sire of Ormonde, and the skeleton of Stockwell, of whom he was a lineal descendant.

Announcement is made by the board of directors of the National Association for the Study and Prevention of Tuberculosis of the preliminary arrangements for the second annual meeting of the association, which will be held in Washington, May 17–19, 1906. Two new sections have been established, one on surgical tuberculosis and the other on tuberculosis in children. The officers of the sections are as follows: Sociological section—chairman, Mr. William H. Baldwin, Washington, D. C.; secretary, Miss Lilian Brandt, New York. Clinical and Climatological section—chairman, Dr. Vincent Y. Bowditch, Boston, Mass.; secretary, Dr. Edwin A. Locke, Boston, Mass. Pathologic and bacteriologic

section—chairman, Dr. Edward R. Baldwin, Saranac Lake, N. Y.; secretary, Dr. Hugh M. Kinghorn, Saranac Lake, N. Y. Section on surgical tuberculosis—chairman, Dr. W. W. Keen, Philadelphia, Pa.; secretary, Dr. Robert G. LeConte, Philadelphia, Pa. Section on tuberculosis in children—chairman, Dr. W. P. Northrup, New York; secretary, Dr. Roland G. Freeman, New York.

THE lectures on tuberculosis at the tuberculosis exposition, to be held in Philadelphia from January 22 to February 3, will be delivered by the following: January 23, Dr. Lawrence F. Flick, 'The Sociological Importance of Tuberculosis'; January 24, Dr. Leonard Pearson, 'State Control of Tuberculosis'; January 25, Dr. Charles Dudley, Altoona, 'The Railroad in Tuberculosis'; January 26, Dr. William B. Stanton, 'Tuberculosis in the School'; January 27, Dr. Howard S. Anders, 'Tuberculosis in the Store'; January 29, Dr. Henry R. M. Landis, 'Tuberculosis in the Workman'; January 30, Dr. Samuel McC. Hamill, 'Tuberculosis in Children'; January 31, Dr. Thomas Darlington, New York City, and Dr. W. M. Late Coplin, 'Municipal Control'; February 1, Dr. Charles J. Hatfield, 'Address to Medical Students and Nurses'; February 2, Drs. James C. Wilson and John H. Musser, 'Address to Physicians,' and February 3, Dr. M. P. Ravenel, 'Hospitals, Sanatoria and Dispensaries.' Demonstrations on pathology will be given from 2 to 6 P.M. daily.

MR. E. H. PLUMACHER, the American consul at Maracaibo, has written the following letter to the assistant secretary of state, under date of December 3.

I should have reported sooner to you about the different convulsions of mother earth in this consular district during the month of November. Excuse circumstances; I was not in a condition to report upon such small affairs, as I had enough to do to save my reputation. We had since the tenth of November two slight ones here but heavy felt in the Andes states. Then we had three sharp convulsions of little duration coming from the southeast.

The days were cloudy and no wind but it was very hot. In day time those shakings are taken rather cooly, but when in night time such freaks of nature occur and you hear the rafters creak and the tiles on your roof move and your bed shakes it is not exactly pleasant. We had them but without much damage. The damage came from an unexpected quarter. It began to rain from 5 A.M. to 12:30 P.M. in such a style that it seems as if tons of water were thrown upon the country. I marked five and a half inches before 10 o'clock A.M. All our houses were floated, no tile roof after the shaking could stand such a flood. I intended to go to the city anyhow at 8:30 in the morning on horse-back to see to the condition of the consular office in town, but it was impossible; a stream of water six feet high with the rapidity of lightning came down the streets and I knew that was more than horse could cross. There was no communication in town that day, but next day I was at six o'clock in the morning at the office only to find three inches of water in all the rooms, not come by flooding but by leaking roofs; not much damage was done in the office; we can with little expense put everything in first class order. The damage done to property by the water is great. For ten years we had no rain sufficient to serve for the agriculture, not speaking of the suffering people, but now it came down in such an abundance that the poor people can drink once more, sweet fresh rain water. The cisterns are full, everybody can now hope for a good crop and there is water enough for the cattle in the plains and plenty food for them in a short time.

The Scottish Geographical Journal gives the following details in regard to the plans of the Mylius Erichsen expedition: The intention is to make a start about the middle of June, 1906, on board the *Denmark*, the party consisting of twenty-one persons, among whom will be a zoologist, a painter, a doctor, a botanist and a biologist. Seventy sledge-dogs and some motor boats are to be taken. The vessel is to be steered for the east coast of Greenland, and it is hoped that a harbor may be found in lat. 70°. From this harbor an expedition consisting of twelve men with sledges and dogs will start in March, 1907, for the extreme north, where its members hope to map part of the unknown coast of eastern Greenland. This party is expected to return in July, 1907, and if conditions are favorable it is contemplated that another party may be sent into the interior, or may even attempt to cross the country from east to west in a high latitude, say along the 70th

or 75th parallel. The expedition is expected to finally return in the summer of 1908, and the sum of £11,000 is given as its probable cost.

UNIVERSITY AND EDUCATIONAL NEWS.

Mr. N. W. Harris, of Chicago, has presented $25,000 to Northwestern University, to be used as an endowment for an annual series of lectures which are to be delivered by some distinguished man, not a professor of the university, upon the results of his own investigations in scientific, literary, economical or theological problems.

By the will of Andrew J. Dotger, of South Orange, N. J., the Tuskegee Normal and Industrial Institute will, at the death of the testator's wife, receive the residuary estate, said to be about half a million dollars.

We learn from *Nature* that the University of Bâsle, to which the late Professor Dr. Georg W. A. Kahlbaum was attached for nearly twenty years, has received the sum of 100,000 francs from the mother of the deceased professor. Further, Professor Kahlbaum's scientific library and physical instruments are also to be handed to the university.

The corporation of Harvard University has decided that students who take more than the required amount of work must in addition to the regular tuition fee of $150 pay $20 for each course. If a student wishes to finish his course in three years, he must take up four additional courses.

The University of Oxford has established a diploma in anthropology, awarding a certificate of merit after written and practical examination at the end of a course of study of not less than a year in residence and under supervision.

The college entrance examination board will hereafter conduct an examination in zoology. The examiners of the board for 1906 in the sciences are as follows:

Mathematics—Professor F. N. Cole, Columbia University; Professor H. S. White, Vassar College; Dr. Arthur Schultze, High School of Commerce, New York, N. Y.

Physics—Professor E. L. Nichols, Cornell University; Professor F. C. Van Dyck, Rutgers College; Frank Rollins, Stuyvesant High School, New York, N. Y.

Chemistry—Professor H. P. Talbot, Massachusetts Institute of Technology; Professor L. M. Dennis, Cornell University; C. M. Allen, Pratt Institute, Brooklyn, N. Y.

Geography—Professor A. P. Brigham, Colgate University; Professor Florence Bascom, Bryn Mawr College; W. H. Snyder, Worcester Academy, Worcester, Mass.

Botany—Professor W. F. Ganong, Smith College; Professor Henrietta E. Hooker, Mount Holyoke College; Louis Murbach, Central High School, Detroit, Mich.

The College of Engineering of the University of Wisconsin announces a list of non-resident lectures for the present year. The lecturers selected include some of the most prominent authorities on special phases of engineering in the country. The lectures are not confined strictly to engineering subjects, but include a consideration of various industrial and commercial problems with which the engineer has to deal, and embrace the greatest possible variety of subjects. The complete program is as follows: Mr. F. B. Wheeler, engineer of the Semet-Solvay Co., Syracuse, N. Y., two lectures, 'Gas Engineering.' Mr. G. M. Davidson, chemist for C. & N. W. Railway, 'Purification of Water for Locomotive Boilers.' Mr. J. M. Faithorn, president of the Chicago Terminal Transfer Co., 'Regulation of Railroad Freight Rates.' Mr. B. A. Behrend, chief engineer of Bullock Manufacturing Co., 'High Speed in Modern Engineering.' Mr. S. Wyer, consulting engineer, Columbus, O., two lectures, 'Gas Producers and Producer Gas.' Mr. L. R. Clauson, U. W. '97, signal engineer of C. M. & St. P. Railway Co., 'Railroad Signaling.' Mr. Ralph Modjeski, consulting bridge engineer, Chicago, 'The New Thebes Bridge over the Mississippi near St. Louis.' Mr. Arthur B. Wheeler, president of the Chicago Telephone Co., subject to be announced later. Professor L. P. Breckenridge, College of Engineering, University of Illinois, 'The Use of Bituminous Coal in Boiler Furnaces.' Mr. Andrews Allen, U. W. '91, construction engineer, Chicago, 'Engineering Construction.' Mr. Frank Skinner, assistant editor of *Engineering Record*, probably several lectures on 'Bridge Construction.'